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ABSTRACT

A collection of essays marking the retirement of Ilse Lehiste, chair of the Department of Linguistics at Ohio State University (1967-71 and 1985-87) includes the following contributions by her colleagues: "On Incomplete Mutations in Breton"; "Perceived P-Center Location in English and Japanese"; "Ordering Paradoxes and Lexical Phonology"; "The Slovenian Orphan Accusative, Component Interfaces, and Covert Grammatical Categories"; "Parameters and Markedness in the Acquisition of Syntax"; "Notes toward a Semantic Simulation of a Fragment of Child Language"; "Some Literary Manifestations of Language Contact"; "'Concessive' as a Discourse Relation in Expository Written English"; "Adverbs, Polysemy, and Compositional Semantics"; "Lake Miwok Naturalization of Borrowed Phonemes"; "What Is 'Karaka'? A Probe into Panini's Analytical Procedure"; "Codeswitching in 'War and Peace'"; "A Greek-Bulgarian Mischsprache in the Rhodope?"; "Feminism in Morphology"; "Paragraph Perception by Seven Groups of Readers"; "Pronouns and People: Some Preliminary Evidence That the Accessibility of Antecedents in Processing Can Vary with Clause Relation and Biology"; "Some Implications of Issues in Social Dialectology for Linguistic Reconstruction"; "The Phonological Domains of Final Lengthening"; "On Situation Adverbs"; "On the Phrasing of Coordinate Compound Structures"; "Closure Duration in the Classification of Stops: A Statistical Analysis"; and "Silent Features and Syntactic Analysis." (MSE)

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No. 35

A FESTSCHRIFT FOR ILSE LEHISTE

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A Festschrift for Ilse Lehiste

Edited by
Brian D. Joseph
and
Arnold M. Zwicky

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Introduction

A Festschrift for Ilse Lehiste

Edited by Brian D. Joseph and Arnold M. Zwicky

This volume celebrates our colleague Ilse Lehiste and also marks the twentieth anniversary of the Department of Linguistics at the Ohio State University. Since Ilse's retirement happens to coincide with this milepost in the history of the department she did so much to shape—as the director of the Division of Linguistics from 1965–1967 and chair of the Department of Linguistics from 1967–71 and 1985–87, and as its most distinguished member—we are able to offer her a doubly appropriate token of our regard.

The contributors to this volume have all been Ilse's colleagues during her years at Ohio State (a full list of those who have held faculty appointments since the Division of Linguistics was established is provided on the next page). Our diverse contributions touch on most of the areas in which Ilse has published during her career: studies in Baltic, Finno-Ugric, Germanic, and Slavic languages; historical and comparative linguistics; instrumental phonetics, investigating both the perception and production of language; language contact; phonological theory; poetics and stylistics; and prosody and suprasegmentals. We look forward to her own contributions in these areas during the department's third decade.

OSU WPL # 35

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On Incomplete Mutations in Breton

Gregory T. Stump
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1. Incomplete mutations

Among the distinctive characteristics of the Celtic languages are their elaborate systems of grammatically conditioned word-initial consonant mutations. Breton, for example, possesses four principal mutations, according to the native grammatical tradition: these are the spirantizing, the reinforcing, the leniting, and the mixed mutations. In a particular syntactic context, the initial consonant of a word may undergo one or another of these mutations, ordinarily as a consequence of the influence of some immediately adjacent mutation 'trigger'. Thus, the possessive pronoun e 'his' lenites the initial consonant of the noun which follows it, as in the examples in (1).

(1)	Lenition:	p	→ b	penn 'head', e benn 'his head'
		t	→ d	tad 'father', e dad 'his father'
		k	→ g	kador 'chair', e gador 'his chair'
		b	→ v	breur 'brother', e vreur 'his brother'
		d	→ z	dant 'tooth', e zant 'his tooth'
		g	→ c'h (= [ɦ])	gavr 'goat', e c'havr 'his goat'
		gw	→ w	gwele 'bed', e wele 'his bed'
		m	→ v	mamm 'mother', e vamm 'his mother'

Similarly, the possessive pronoun va 'my' spirantizes the initial consonant of the noun which follows it, as in the examples in (2).

(2)	Spirantization:			
		p	→ f	penn 'head', va fenn 'my head'
		t	→ z	tad 'father', va zad 'my father'
		k	→ c'h (= [x])	kador 'chair', va c'hador 'my chair'

(Strictly speaking, spirantization should be viewed as converting voiceless stops into voiceless fricatives; the latter, however, may subsequently undergo a phonological rule which voices initial fricatives after resonants. The effects of the latter rule are often simply regarded as an integral part of the spirantization process, since all of the spirantization triggers in Breton end in resonants. The contexts in which the fricative voicing rule applies vary dialectally; in the orthography of standard literary Breton, t is the only initial consonant whose spirant alternant is explicitly represented as voiced. See Jackson (1967: 360–375) and Willis (1982: 24f, 114ff) for discussion.)

In traditional grammars of Breton, certain expressions are claimed to trigger incomplete mutations; an incomplete mutation is just like one or another of the principal mutations except that it apparently affects a smaller range of consonants. For example, the first person singular enclitic pronoun 'm is, in many dialects, said to trigger an incomplete spirantization, in that it appears to spirantize t and k but not p:

(3) a. kambr 'room' (e 'in' + 'm →) em c'hambr 'in my room'
ti 'house' em zi 'in my house'
but: penn 'head' em penn 'in my head'
b. karout 'like' (da 'to' + 'm →) da'm c'harout 'to like me'
treiñ 'turn' da'm zreiñ 'to turn me'
but: prenañ 'buy' da'm prenañ 'to buy (for) me'

This pattern is typical of the standard literary dialect (Kervella (1947: 95)) and the Léon dialect (Vallée (1926: 69, fn 1), Hemon (1975a: 8)), and is reported as optional in other dialects (e.g. by Le Roux (1896: 8f), Trépos (n.d.[1968]: 46), and Vallée (1926: 79, fn 1)). In Trégorrois, however, 'm triggers the full range of spirantizations (Le Clerc (1911: 19), Le Roux (1896: 8ff)).

Despite this variation in the pattern exemplified in (3), other cases of incomplete mutation appear exceptionlessly in all dialects of the language. For example, when an article is immediately followed by a feminine singular noun (or by a masculine plural noun with human reference), the article apparently triggers the full range of lenitions except that of d to z:[1]

(4)

	paner	'basket'	ar baner	'the basket'
	taol	'table'	an daol	'the table'
	kador	'chair'	ar gador	'the chair'
	bag	'boat'	ar vag	'the boat'
	gavr	'goat'	ar c'havr	'the goat'
	gwern	'mast'	ar wern	'the mast'
	mamm	'mother'	ar vamm	'the mother'
but:	delienn	'leaf'	an delienn	'the leaf'

Thus, in a formal analysis of Breton mutations, one might postulate the existence of a partial lenition rule (5b) alongside the full lenition rule (5a); and for those dialects other than Trégorrois, one might postulate a partial spirantization rule (6b) alongside the full spirantization rule (6a).

(5) a. Lenition:

$$\begin{bmatrix} -\text{son} \\ +\text{voi} \end{bmatrix} \rightarrow [+\text{cont}]$$

$$\begin{bmatrix} -\text{son} \\ -\text{cont} \end{bmatrix} \rightarrow [+\text{voi}]$$

$$\begin{bmatrix} +\text{nas} \\ +\text{ant} \\ -\text{cor} \end{bmatrix} \rightarrow [+\text{cont}]$$

} in leniting environments (cf. Willis (1982: 54f))

b. Partial lenition:

$$\begin{bmatrix} -\text{son} \\ -\text{cor} \\ +\text{voi} \end{bmatrix} \rightarrow [+\text{cont}]$$

$$\begin{bmatrix} -\text{son} \\ -\text{cont} \end{bmatrix} \rightarrow [+\text{voi}]$$

$$\begin{bmatrix} +\text{nas} \\ +\text{ant} \\ -\text{cor} \end{bmatrix} \rightarrow [+\text{cont}]$$

} after the articles [in certain contexts], ...

(6) a. Spirantization: [- voi] → [+ cont] in spirantizing environments (cf. Willis (1982: 57))

(6) b. Partial spirantization: $\begin{bmatrix} -\text{ voi} \\ \alpha\text{ ant} \\ \alpha\text{ cor} \end{bmatrix} \rightarrow$ [+ cont] after 'm, ...

Hereafter, I shall refer to this kind of analysis of incomplete mutations as the partial mutation (or PM) analysis.

2. Incomplete mutations as the effect of mutation reversal

Willis (1982: 119-121) has argued that in certain cases, expressions which have traditionally been analyzed as triggering incomplete mutations should in fact be viewed as triggering complete mutations; in such cases, she claims, the mutations only appear to be incomplete because some of their effects are reversed by low-level phonological rules.

Consider the class of nouns which includes both feminine singulars and masculine plurals with human reference (a class to which I shall henceforth refer as 'FS/MPH nouns'): nouns in this class ordinarily trigger a lenition in a following adjective, as in (7a-c); those which end with an obstruent, however, seemingly fail to trigger a lenition if the following adjective begins with a voiceless stop. Thus, the feminine singular nouns in (7d-f) apparently trigger an incomplete lenition (one which is distinct from that triggered by the definite article in (4)):

(7)	a.	paour	'poor'	ur vamm baour	'a poor mother'
	b.	tev	'thick'	ur wern dev	'a thick mast'
	c.	kaer	'fine'	ur gador gaer	'a fine chair'
	d.			ur vaouez paour	'a poor woman'
	e.			ur voest tev	'a thick box'
	f.			ur gazeg kaer	'a fine mare'

Willis suggests, however, that the apparent failure of lenition in examples such as (7d-f) should instead be viewed as the cumulative effect of lenition and either of two rules of Breton phonology:

(8) a. Obstruents are devoiced following voiceless obstruents.
b. Sequences of two voiced obstruents may optionally stay as they are or be mutually devoiced. (Willis (1982: 119))

In Willis' analysis, the combination of boest 'box' with tev 'thick' produces (7e) in two steps: first, lenition yields ur voest dev, which (8a) then converts to (7e). Similarly, the apparent absence of lenition in (7d,f) is regarded as the cumulative effect of lenition and rule (8b), respectively: ur kazeg kaer lenites to ur gazeg gaer, which (8b) then converts to (7f) (in which the final g in gazeg is voiceless, despite its spelling). In what follows, I shall refer to this sort of approach to incomplete mutations as the mutation reversal (or MR) analysis.

The MR analysis provides a satisfying alternative to the PM analysis in its account of the mutation pattern exemplified in (7). Willis has, however, suggested that two other apparent cases of incomplete mutation should likewise be treated as involving complete mutations whose effects are partially reversed by low-level phonological rules. I shall argue here that for these latter two cases, the MR analysis is poorly motivated.

Consider again the examples in (3) and the traditional view that 'm triggers an incomplete spirantization which leaves p unaffected. Willis rejects this view, proposing instead that 'm triggers the full range of spirantizations but that the mutation of p to f is subsequently reversed by an assimilative phonological rule converting f to p after m; according to her proposal, em penn derives from the underlying sequence e+'m penn by spirantization (→ em fenn) followed by assimilation.

Willis proposes a similar account of the apparently incomplete pattern of lenition exemplified in (4); that is, she suggests that the articles an, un trigger the full range of lenitions but that the mutation of d to z is ultimately reversed by an assimilation converting z to d after n. Thus, an delienn derives from the underlying sequence an delienn by lenition (→ an zelienn) followed by assimilation.

The plausibility of this account of the mutation patterns in (3) and (4) is, of course, entirely dependent on the extent to which one can justify postulating a phonological rule whose effect is to reverse the spirantization of p and the lenition of d. Willis does not explicitly formulate such a rule. (9), however, might be proposed as a rule achieving the desired effect; note that as (9) is stated, it must be ordered before the fricative voicing rule mentioned above if it is to reverse the spirantization of p in the intended manner.

(9) $$\begin{bmatrix} -\text{son} \\ +\text{ant} \\ \alpha\text{cor} \\ \alpha\text{voi} \end{bmatrix} \rightarrow [-\text{cont}] \; / \; \begin{bmatrix} +\text{nas} \\ +\text{ant} \\ \alpha\text{cor} \end{bmatrix} \; \# \; ____$$

As it stands, rule (9) (hence, the MR analysis itself) turns out to be implausible for two reasons; moreover, it is not clear that (9) can be modified in such a way as to overcome these two difficulties, as I shall show in the following two sections.

3. A potential problem for the MR analysis: underlying initial f

If (9) is in fact a rule of Breton phonology, then it should apply not only to instances of f and z arising as the effect of a mutation--it should, in addition, apply to any underlying instance of initial f or z preceded by a homorganic nasal. This prediction is difficult to confirm (or to disconfirm) in the case of z, since only a vanishingly small number of words begin with an underlying z in Breton, and nearly all of these are obviously recent borrowings. Words with underlying initial f are nevertheless abundant, and it is clear that the initial f in such words does not become p when preceded by m; this is true whether the preceding expression is a spirantization trigger (as in (10)) or not (as in (11)).

(10) a. em fri 'in my nose', da'm frealziñ 'to console me'
b.(*)em pri (≠ 'in my nose'; = 'in my mud (pri)'), *da'm prealziñ

(11) a. en em flojenniñ 'to find shelter' (en em: reflexive particle), ur vamm fat 'a mother overcome with fatigue'
b. *en em plojenniñ, *ur vamm pat

Thus, (9) cannot be adopted in its present form, since it would incorrectly

convert the a examples in (10) and (11) to the corresponding b examples.

A proponent of the MR analysis could, however, react to this evidence with a counterargument: that at the stage at which rule (9) applies, the spirantized alternant of p is in some way distinguished from underlying f. Consider again the spirantization rule in (6a): by itself, (6a) predicts that the spirant alternant of p should be [ɸ]. This prediction could be overridden by adopting the redundancy rule in (12), which would automatically require f rather than [ɸ] as the spirant alternant of p; but suppose, on the other hand, that (12) were instead a low-level rule applying after (6a) to convert [ɸ] into f.

(12) $\begin{bmatrix} - \text{ son} \\ + \text{ cont} \\ + \text{ ant} \end{bmatrix} \rightarrow$ [+ stri] (cf. Willis (1982: 55))

Under this latter assumption, the failure of rule (9) to apply in the examples in (10) and (11) could then be easily accounted for by (i) restricting the application of rule (9) to segments marked [- stri], as in (9');

(9') $\begin{bmatrix} - \text{ son} \\ - \text{ stri} \\ + \text{ ant} \\ \alpha \text{ cor} \\ \alpha \text{ voi} \end{bmatrix} \rightarrow$ [- cont] / $\begin{bmatrix} + \text{ nas} \\ + \text{ ant} \\ \alpha \text{ cor} \end{bmatrix}$ # ____

(ii) ordering rule (9') after the spirantization rule (6a) but before the low-level rule (12); and (iii) assuming that underlying f is indeed [+ stri], as its spelling suggests.[2] According to this analysis, the expressions va fenn 'my head', em penn 'in my head', and em fri 'in my nose' would be derived as in (13).

(13) Underlying form:	va penn	em penn	em fri
(6a):	va ɸenn	em ɸenn	—
(9'):	—	em penn	—
(12):	va fenn	—	—

This analysis depends (a) on the existence of a phonological distinction between [ɸ] and f at the stage at which (9') applies, and (b) on the subsequent, absolute neutralization of this distinction by rule (12). To my knowledge, however, there is no independent motivation for regarding (12) as anything other than a redundancy rule, incapable of interacting with other rules as though it were itself an ordered rule; thus, in any reasonably concrete approach to phonology, the proposed revision of the MR analysis would have little to recommend it over the PM analysis.

One could, in fact, imagine a sort of compromise between the PM and MR analyses which would provide a superior account of the incomplete spirantization triggered by 'm, and would do so without recourse to rule (9'). In this analysis, 'm would still be regarded as an ordinary spirantization trigger, but the applicability of this mutation would be subject to the following anti-dissimilation condition in those dialects showing the mutation pattern in (3):

(14) A mutation rule has no effect if it would cause a [- son, + ant, α cor, α voi] segment to become [+ cont] after a [+ nas, + ant, α cor] segment.

Under this analysis, 'm would be treated as a spirantization trigger on a par with va 'my'; unlike va, however, 'm would be incapable of spirantizing p in those dialects subject to restriction (14). This account (which I shall call the conditional mutation (or CM) analysis) is superior to the revised MR analysis in that it doesn't entail the postulation of any absolutely neutralized phonological distinctions, nor does it require one to view (12) as anything other than a redundancy rule; and the fact that underlying initial f remains unaffected when preceded by 'm follows, in the CM analysis, from the simple fact that fricatives aren't mutable consonants in Breton.

The CM analysis might appear to be indistinguishable in its predictions from the PM analysis, at least as far as the spirantizing properties of 'm are concerned; there is one important difference, however. As mentioned above, spirantization of p after 'm is optional in some dialects of Breton; in Vannetais, for example, both (15a) and (15b) are possible:

(15) a. ean em prenas
he bought (e (verbal particle) + 'm → em)
'he bought (for) me'

b. ean em frenas (Guillevic & Le Goff (1912: 8))

This optionality is easily accounted for under the assumptions of the CM analysis: one can simply regard (14) as an optional tendency (rather than an absolute restriction) in Vannetais and similar dialects. In the PM analysis schematized in (6b), on the other hand, it is not clear how the optionality exemplified in (15) might be accounted for. Perhaps one could assume that 'm may function alternatively as a full-fledged spirantization trigger or as a trigger of partial spirantization; I know of no indisputable precedent, however, for such free variation in the properties of mutation triggers.[3]

To summarize: three different approaches to the incomplete mutation produced by 'm have been examined in this section. The success of the MR analysis hinges on the validity of the phonological rule (9'); maintaining this rule, however, entails the postulation of an absolutely neutralized distinction between f and [ɸ] in Breton—a distinction which might be rejected on metatheoretical grounds. The PM analysis, embodied by rule (6b), provides no ready account of the fact that in some dialects, initial p may optionally appear in its spirant form after the spirantization trigger 'm. The CM analysis, like the MR analysis, makes no use of partial mutation rules such as (6b); but by employing (14) as a phonological condition on the application of full mutation rules such as (6a), the CM analysis avoids the metatheoretical objections to the MR analysis, and also provides a natural means of accounting for the optional spirantization of p after 'm in certain dialects. I therefore conclude that the CM analysis provides a superior account of the incomplete pattern of spirantization exemplified in (3).

In the following section, I shall present some additional evidence against the revised MR analysis; in particular, I shall argue that the pattern of incomplete lenition in (4) cannot be the effect of mutation reversal.

4. A second problem for the MR analysis: optional lenition of d after n

The revised MR analysis developed above predicts (I) that the spirantization of p is uniformly reversed after spirantization triggers ending with m; and (II) that the lenition of d is uniformly reversed after lenition triggers ending with n. The attractiveness of the revised MR analysis therefore hinges on whether or not these predictions are actually confirmed. Prediction (I) is confirmed, but only trivially so, given that the first person singular clitic pronoun 'm is the only spirantization trigger which ends with m (except in Trégorrois; vide infra). Prediction (II), on the other hand, is actually disconfirmed, as I shall now show.

In most dialects of Breton, there are three kinds of lenition triggers which end with n:[4] (a) the articles an 'the', un '@'; (b) the preposition dindan 'under'; and (c) FS/MPH nouns ending with n.[5] According to prediction (II) above, the lenition of d should be uniformly reversed after all of the lenition triggers in (a)-(c). It is universally true in Breton that initial d never surfaces as z when preceded by an article; but both within and across dialects of Breton, there is considerable variation in the behavior of initial d when it is preceded by the lenition triggers in (b) and (c).

Consider, for example, the preposition dindan. Kervella (1947: 85), a native of northwestern Cornouaille, asserts that d may optionally appear in its lenited form after dindan, as in (16b).

(16) a. dek 'ten' b. dindan zek devezh 'in ten days'

Hemon (1975b: 12-14), a native of Brest (in the dialect region of Léon), observes that dindan may sometimes fail to produce any lenition--whether of d or of any other consonant--but classifies it among the lenition triggers which may convert d to z rather than among those which leave d unaffected; cf. also Vallée (1926: 101; 102, fn 2). Thus, in those dialects in which dindan acts as a lenition trigger,[6] it doesn't affect initial d in the same way as the articles do: in all such dialects, dindan may lenite d to z in at least some circumstances.

Now consider lenition triggers of type (c)--FS/MPH nouns ending with n. According to Vallée (1926: 114), nouns of this sort lenite the initial d of a following modifier in the Léon dialect, as in (17b); Hemon (1975b: 17) regards the lenition of initial d by any sort of FS/MPH noun as optional in this dialect, but cites (18b) and (19b) as examples in which d is lenited to z after n. (I have standardized the spelling in these examples.)

(17) a. dall 'blind' b. al logodenn zall 'the blind mouse (= bat)'
(18) a. diaouloù 'devils' b. ur vandenn ziaouloù 'a crowd of devils'
(19) a. derv 'oak' b. ur c'hrizienn zerv 'the root of an oak'

Similarly, Trépos (n.d.[1968]: 37-38), a native of southwestern Cornouaille, cites the examples in (20b) and (21b):

(20) a. du 'black' b. un delienn zu 'a black leaf'
(21) a. dir 'steel' b. ur bluenn zir 'a steel pen'

Finally, Kervella (1947: 90-91) asserts that the lenition of initial d after a FS/MPH noun ending in n is optional in standard literary Breton.

In the Tréguier dialect, initial d is never lenited after a FS/MPH noun ending in n; this is, however, merely one reflection of the fact that initial d is never subject to lenition in this dialect--not even if it is preceded by a lenition trigger ending in a sound other than n (Hemon (1975b: 6), Jackson (1967: 313), Kervella (1947: 91), Le Roux (1896: 17)).[7] Thus, in those dialects in which postnominal adjectives are productively lenited by FS/MPH nouns and in which d is lenitable, FS/MPH nouns ending with n don't affect initial d in the same way as the articles do: in all such dialects, nouns of this sort may optionally lenite d to z.

Consider the possibilities which are open at this juncture. Certainly it cannot be maintained 'that the lenition of d is uniformly reversed after lenition triggers ending with n'; prediction (II) of the MR analysis is plainly disconfirmed, both by the leniting properties of dindan and by those of FS/MPH nouns ending with n. Whether a lenition trigger with a final n lenites a following initial d or leaves it unaffected thus depends not merely on the phonological context, but on the grammatical identity of the trigger; this being the case, the pattern of incomplete mutation in (4) cannot simply be viewed as the cumulative effect of the lenition rule (5a) and the phonological rule (9'). The revised MR analysis must therefore be rejected.

How, then, is the mutation pattern in (4) to be accounted for? More specifically, how can one account for the fact that in those dialects in which it is lenitable, initial d absolutely resists lenition when preceded by an article, but may optionally undergo lenition when preceded by any other lenition trigger ending with n?

Clearly the articles are somehow differentiated from the other lenition triggers ending with n. What distinguishes the articles, I suggest, is that they trigger the partial lenition in (5b); this accounts for their absolute failure to lenite d in any dialect of the language. On the other hand, I suggest that the remaining lenition triggers ending with n trigger the full set of lenitions in (5a), but are subject--optionally--to the phonological restriction (14) (except in Trégorrois, to which I return presently). Thus, my proposal is that the peculiar difference between the articles and the other n-final lenition triggers can best be accounted for by a combination of the PM analysis with the CM analysis: the former accounts for those cases in which the lenition of d after n is absolutely blocked (i.e. after the articles), while the latter provides for those cases in which this same lenition exists at least as an option (i.e. after other lenition triggers ending with n).

Let me note, in conclusion, that this mixed approach to the lenition of d after n provides a much more satisfying account of the peculiarities of Trégorrois than the MR analysis does. Recall that in the dialect of Tréguier, d never undergoes lenition under any circumstances. To account for this fact, one must assume that lenition is a narrower phenomenon in Trégorrois than in the other Breton dialects--that it is, in fact, identical in its effects to the 'partial lenition' represented in (5b). A proponent of the MR analysis would therefore have to assume that Trégorrois possesses a 'full lenition' rule distinct from any rule found in the other dialects (cf. Willis (1982: 156, fn 6)); and even though d is never lenited after the articles in any dialect of Breton, the proponent of the MR analysis would have to view this fact as the effect of different rules in different dialects--in Léonais, it would be viewed as the cumulative effect of lenition (rule (5a)) and mutation

reversal (rule (9')), while in Trégorrois, it would be viewed as an effect of the atrophied lenition rule (= (5b)) peculiar to that dialect.

The analysis proposed here affords a much more satisfying understanding of the dialect of Tréguier. In my analysis, the full lenition rule in Trégorrois is identical to the partial lenition rule proposed for the other dialects; as a consequence, the former can be viewed as a straightforward analogical development from the latter (Jackson (1967: 313), Le Roux (1896: 17)). Moreover, the fact that d is never lenited after the articles in any dialect receives a single, unified explanation in the analysis proposed here: this fact follows directly from the assumption that in all dialects, the articles trigger the pattern of lenitions in (5b).

Because d isn't lenitable in any context in Trégorrois, condition (14) is obviously irrelevant as a constraint on lenition in this dialect. As it turns out, it is irrelevant for spirantization as well. Recall first that in Trégorrois (unlike the other dialects), the enclitic 'm triggers the full range of spirantizations, even that of p to f. In addition, Trégorrois (again unlike the other dialects) possesses a second spirantization trigger ending with m, namely the possessive pronoun hom 'our' (Le Roux (1896: 9-10), Trépos (n.d.[1968]: 46)); this, too, triggers the full range of spirantizations. Accordingly, condition (14) is simply irrelevant for the analysis of incomplete mutations in Trégorrois. Thus, while I have proposed a mixed PM/CM analysis for most Breton dialects, it appears to be most appropriate to account for all incomplete lenitions in the dialect of Tréguier by means of the PM approach.

Notes

1. Note that the Breton articles are subject to the following phonologically conditioned alternation: un, an appear before initial n, d, t, h, or an initial vowel; ul, al appear before initial l; and ur, ar appear elsewhere.

2. In fact, graphic f,v represent bilabial (hence [- strid]) fricatives in at least some dialects of Breton; cf. Hemon (1975a: 84). In order to adapt the MR analysis to these dialects, each of (6a), (9'), and (12) would have to be reformulated in some way.

3. There are, of course, expressions that may trigger different mutations in complementary contexts; e.g. the articles, which trigger an incomplete lenition in a following FS/MPH noun (as in (4)), but trigger the incomplete spirantization of k to c'h in a following noun not belonging to the FS/MPH class. This is very different from a situation in which a particular word could freely trigger either of two different mutations in the same context.

4. For brevity's sake, I am excluding from consideration those instances of lenition occurring in the internal morphology of words, as, for example, in compounds; note, however, that the lenition of d after n is not unusual in such combinations: kornzigor 'ajar' (← korn 'corner' + digor 'open'); dindan-zouar 'underground' (← dindan 'under' + douar 'earth'); kenziskibl 'classmate' (← ken- (expresses association) + diskibl 'pupil').

5. According to the discussion in Guillevic & Le Goff (1912), the Vannes dialect does not possess a lenition trigger of type (b): in this dialect, the preposition dindan 'under' appears as edan, and does not produce any sort of mutation. Moreover, only a handful of frequently used adjectives undergo lenition after FS/MPH nouns in the Vannes dialect, and as it happens, none of these adjectives begins with d. Thus, as far as this dialect is concerned, prediction (II) is borne out, but again, only trivially, since the articles are the only lenition triggers which end with n and can precede an initial lenitable d.

6. To judge from the discussion in Le Clerc (1911) and Le Roux (1896), dindan never functions as a lenition trigger in the dialect of Tréguier; cf. also footnote 5.

7. Apparently d did at one time undergo lenition in Trégorrois; early in this century, in fact, Le Clerc (1911: 17, 21f) still classified the lenition of d as an option in certain circumstances.

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Perceived P-Center Location in English and Japanese*

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1.0 Introduction

To most speakers/hearers, both linguistically trained (Abercrombie, 1964; Classe, 1939; Pike, 1945) and naive (Donovan & Darwin, 1979; Lehiste, 1972), languages sound rhythmical, that is, the occurrence of spoken elements in time seems to be organized in terms of some potentially specifiable set of principles. Three classes of rhythmic organization have been proposed for languages: stress-timing (e.g., English, German), syllable-timing (e.g., French, Spanish), and mora-timing (Japanese). In stress-timing, for example, the temporal regulation of the spoken utterances should make the intervals between stressed syllables approximately isochronous. However, research directed at the nature of the temporal characteristics of speech, particularly for English (the most studied language in terms of its rhythmical properties), has failed to discover strict regularity or isochrony between acoustically defined intervals--such as stressed syllable onset--in either spontaneous or more constrained utterances (e.g., Classe, 1939; Shen & Peterson, 1962; Bolinger, 1965; Lehiste, 1972; see discussion in Lehiste, 1982).

The perception of rhythmicity in speech also does not seem to arise from the presence of isochronous acoustic onsets of linguistic elements (such as stressed syllables). For example, if listeners hear acoustically isochronous sequences of monosyllables (i.e., equal temporal intervals between the syllables' acoustic onsets) whose initial consonants differ in terms of manner of articulation these sequences will sound irregular. Listeners will hear these sequences as being "regular" only if systematic deviations from acoutic isochrony are introduced (Morton, Marcus, & Frankish, 1976; Fowler, 1979, 1981; Fowler & Tassinary, 1981). Fowler (1979) found that the temporal deviations from isochrony that appeared in the speech of talkers attempting to produce isochronous sequences of speech were precisely those anisochronies required by listeners to perceive the utterances as regular.

It is thus apparent that listeners and talkers are capable of focusing on some aspect of orally produced speech when required either produce speech or to make timing judgments. A question that remains is determining upon what basis listeners/speakers on making their timing judgments. Morton et al. (1979) introduced the term "perceptual center" or "P-center" which was defined as the perceptual moment of occurrence of a monosyllabic token [1]. Regular sequences of speech tokens have, by definition, perceptually isochronous P-center. The P-center thus defined presumably corresponds to the locus of the "stress beat" [2] (Allen 1972; Rapp, 1972). The P-center, however, does not seem to correspond to any commonly measured acoustic event such as the onset of

measurable acoustic energy, the onset of the periodic energy of the stressed vowel, or the energy peak (Rapp, 1971; Fowler, 1979; Tuller & Fowler, 1981). Rather, the P-center in stressed syllables corresponds to some event in the signal which can be affected by the duration of the initial consonant (Fowler, 1979), the durations of the medial vowel and final consonants (Marcus, 1981; Smith & Fowler, 1984; Fox & Lehiste, 1985a,b,c), as well as by the addition of unstressed prefixes and/or suffixes (Fox & Lehiste, 1986).

Fowler and her colleagues (e.g., Fowler, 1979; 1983; Fowler & Tassinary, 1981; Tuller & Fowler, 1980; Smith & Fowler, 1984) have suggested that the P-center may correspond to an articulatory event, such as the onset of the vowel. Since coarticulatory phenomena may blend the acoustic characteristics of the vowel with surrounding consonants this articulatory onset may not line up conveniently with commonly used acoustic measurements (such as onset of vocalic periodicity). As Fowler argues, this may produce the situation in which the acoustic measures deviate from isochrony, even in the event of articulatory isochrony. In particular, the articulatory onset of the vowel may occur during the production of the preceding consonant (particularly with segments such as fricatives, see discussion coarticulatory overlap in Fowler, 1983). This hypothesis corresponds well to the findings of experiments which required subjects to mark perceived stress beats in repeated syllable sequences by finger taps (Allen, 1972a,b; Van Katwijk & van den Berg, 1968) or click location manipulations (Eggermont, 1969; Rapp, 1971). When the stimulus syllable began with a stop, the listeners tended to mark the stress beat as occurring at or near the onset of the vowel. However, when the initial consonant was a fricative and longer in duration, the beat was perceived as occurring earlier in relation to the onset of the vowel's periodicity.

The perceived stress beat does not seem to be related to articulatory onset in a simple manner, however. For example, Marcus (1981) demonstrated that increasing the duration of the [t] closure in the token eight--which would presumably not affect the perception of the articulatory onset of the vowel--shifted the perceived location of the token's stress beat. For the purposes of this paper it is sufficient to state that the phonetic structure of the entire word may contribute to the location of the stress beat.

Given that stress-timing is but one possible principle in the organization of speech rhythm, one obvious concern is with the status and/or nature of stress-beat (or P-center) location in languages using different timing principles. In particular, is the P-center a universal phenomenon? If so, is the location of the P-center determined by the same set of acoustic and/or articulatory cues?

The suggestion that the P-center phenomenon was universal in spoken language behavior was made by Hoequist (1983a) who conducted a study examining the P-center effect in the production of English, Spanish, and Japanese monosyllables. Hoequist required sets of subjects to produce a series of rhythmic utterances. Each utterance was composed of 10 alternating monosyllables that differed in terms of their initial

consonant (an experimental design similar to Fowler, 1979 and others). The stimulus syllables included a, ma, ba, pa, and sa, although only the pairs a-ba, ma-ba, and pa-sa (in both orders) were used in the test utterances. Subjects uttered these alternating sequences in time to a metronome for practice (no information about rate was given) and in the test condition uttered the sequences without external timing cues. The utterances were analyzed in terms of both the durations of the nine intersyllabic intervals (ISIs) in each sequence and the duration of any portion of the syllable preceding vocalic periodicity.

Hoequist (1983a) compared average difference in duration for adjacent ('different onset') ISIs (e.g., pa-sa vs. sa-pa) with the difference for non-adjacent ('same onset') ISIs (e.g., pa-sa vs. pa-sa). The pattern of results indicated that the P-center came after the acoustic onset of the syllable. Examination of the different-onset ISIs in terms of the onset of vocalic periodicity showed that the P-center came before, although much closer to, the onset of periodicity. Analysis of the duration differences showed a significant effect of Onset Type (same vs. different) but no effect associated with Language (English vs. Japanese vs. Spanish). There was also no significant Language X Onset interaction. Hoequist suggested that the P-center effect was present in all three languages investigated, apparently to the same degree. In general, any speaker who attempted to produce isochronous syllables aligned some point in the token which did not correspond either to the acoustic onset of the syllable or the onset of the periodicity.

The question which this paper poses is whether the perceived location of the P-center or stress beat is also generalizable across distinct language groups. To address this question a perceptual experiment was conducted comparing the responses from a group of functionally monolingual Japanese speakers with a group of monolingual American English speakers. In particular, Smith & Fowler (1984), and Fox & Lehiste (1985a,c) demonstrated that the nature of the final consonant in CVC monosyllables affected the location of the stress beat (or P-center) when subjects were required to produce sequences of monosyllabic tokens in both metronome and non-metronome conditions. The present experiment examines whether analogous syllable-final variations can shift the location of the P-center in monosyllables in a perceptual task, and whether such shifts are the same for both American English and Japanese speakers.

2.0 Method

2.1 Subjects

There were 29 monolingual American English subjects. These subjects were undergraduate students at The Ohio State University who participated to fulfill a course requirement in Speech & Hearing Science. There were 31 native Japanese subjects. These subjects were second-year students in the Domestic Science Department at a women's junior college in Tokyo. The instructions for the Japanese subjects were in Japanese and the test was administered by a native Japanese Professor (Dr. Morio Kohno).

2.2 Stimulus Materials

Eleven stimulus tokens were constructed, each of which had the form [da__]. Ten tokens ended in a coronal consonant and one token ended with the vowel [a]. The stimuli consisted of the following: dah, dot, dodd, doss, dozz, dosh, dotch, dodge, don, doll, and dar. A male talker (RAF, a phonetician) produced several examples of each token in time with a metronome pulse which occurred every 1000 ms. The tokens were recorded with a high-quality cassette recorder (Sony TC-FX705) using a condenser microphone (Sony ECM-170) while the talker sat in a sound-conditioned booth (IAC). The metronome pulse was used as an organizing cue and was not recorded. These productions were then low-pass filtered at 4800 Hz and digitized at a 10 kHz sampling rate using the ILS waveform analysis programs implemented on a PDP 11/23 computer. One example of each token was selected for editing. For each token, all acoustic energy prior to the release of the initial [d] consonant was eliminated and the durations of the medial vowel and final consonant were measured. Final stops were released and their durations were measured from consonant closure to closure release. The overall amplitudes of the tokens were then equalized. The vowel, consonant, and vowel+consonant durations for these 11 tokens appear in Table 1.

Table 1. Acoustic measurements, including medial vowel and final consonant duration, and probit-determined means for listener-perceived isochronous ISIs for each of the 11 stimulus tokens, in ms. (Note, the sonorant consonants [r] and [l] are considered as part of the vowel in the following table, and in the accompanying analyses.)

Token	Vowel Duration	Consonant Duration	Probit-determined ISIs	
			English Subjects	Japanese Subjects
dah	451	0	975	965
dodd	388	144	999	974
dot	248	98	1047	1071
dozz	408	124	996	982
doss	272	237	1006	1020
dosh	303	251	996	1020
dotch	226	269	1060	1077
dodge	342	194	1003	999
don	388	147	993	961
doll	466	0	977	960
dar	404	0	1010	951

2.3. Procedure

The experimental procedure utilized was based on that used by Halpern & Darwin (1982). In each separate experimental trial there were four experimental tokens. The first three tokens were dah while the fourth token was one of the 11 tokens listed above. On each trial the

intersyllable interval (the syllable-onset to syllable onset interval) between the first, second, and third tokens was 1000 ms. The ISI between the third and fourth tokens varied from trial to trial. This interval deviation amounted to 0%, 3%, 6%, and 9% of the basic 1000 ms ISI. Since each deviation could be either longer or shorter than the baseline, there was a total of 11 different ISIs for the final interval. The deviation increments were based on difference limens estimated by Halpern & Darwin (1982). The presentation order of these sequences were then randomized, example stimulus sequences and fillers were added and the stimului were converted into analog form, filtered at 4800 Hz and recorded on a high-quality stereo cassette recorder (Sony TC-FX705).

For each trial, listeners were required to listen to the four tokens presented in sequence and to respond whether the final token occurred 'too early' or 'too late.' The experiment was conducted in one session which lasted about 25 minutes. This procedure will not determine the absolute location of a token's P-center but rather will allow a determination of each token's relative P-center location using probit analysis of the resulting psychometric function as compared with the other 10 tokens. This procedure will thus allow us to compare whether vowel and final consonant durations affect the relative location of the P-center in both American English and Japanese speakers.

3.0 Results and Discussion

The data for each stimulus token were collapsed over listeners in each of the two language groups and psychometric functions were derived for each token by plotting the number of sequences in which the fourth token was judged 'late' as a function of the variable ISI interval. These data were then submitted to probit analysis (Ray, 1982) which fitted a normal ogive to each different function. Shown in Table 1 are the means of the fitted distributions for each of the 11 stimulus tokens for each of the two language groups. These means represent an estimate of the ISI required between the third and fourth token so that all four tokens are perceived as occurring isochronously. If we assume that subjects are making their judgments on the basis of aligning the P-center of the four stimulus tokens in time, then the longer the estimated mean ISI to produce isochrony, the earlier the location of the P-center in the fourth stimulus token. These data will be further analyzed first by separate language group to determine the best predictor(s) of estimated isochronous ISIs and then together using analysis of variance to determine whether the two different groups produced significantly different responses.

The English data were analyzed using step-wise multiple linear regression analysis with estimated isochronous ISI values as the dependent variable and vowel duration, consonant duration, and vowel+consonant duration as the independent variables. Regression analysis showed that the ISI values were significantly predicted by vowel duration (r=0.844, $F(1.9)$=22.2, $p<.002$). The slope of the regression line was -0.27. This suggests that as vowel duration increases by 100 ms, the ISI duration needed to produce a perceptually isochronous sequence decreases by 27 ms. This value is only slightly smaller than as those obtained by Smith & Fowler (1984) and by Fox &

Lehiste (1985b,c) who examined the effect of medial vowel duration on P-center location. The present regression results support the conclusion that as the vowel duration increases--as a function of the final consonant--the P-center location moves to a later point in the token. ISI means were also significantly related to final consonant duration ($r(11)=0.62$, $p<.02$), but final consonant duration is also significantly related to vowel duration ($r(11)=-0.90$, $p<.001$). If the contribution of vowel duration is partialled out from the consonant duration variable, consonant duration is only marginally related to mean ISI ($t(10)=-2.2$, $p<.067$).

The estimated isochronous ISI data for the Japanese subjects were also analyzed using step-wise multiple linear regression. Analysis showed that estimated ISIs were significantly predicted by vowel duration ($r=0.93$, $F(1,9)=57.4$, $p<.001$). The slope of the regression line was -0.50. This suggests that as vowel duration increases 100 ms, the ISI duration needed to produce an isochronous sequence decreases by 50 ms. This value is greater than that obtained both for the American English group and by Smith & Fowler (1984) and Fox & Lehiste (1985b,c). Estimated ISIs were also significantly related to final consonant duration ($r(11)=0.81$, $p<.001$), but when the contribution of vowel duration is partialled out, consonant duration is not even a marginally significant predictor of mean ISI ($t(10)=-0.56$, $p>.59$). The basic pattern of results is the same between the two language groups, namely, as vowel duration increases, the P-center location moves to a later point in the token. The similarity between the two groups is best illustrated by the fact that the estimated ISIs between the English and Japanese groups are significantly correlated ($r(11)=0.85$, $p<.001$) although there seems to be some difference between the groups in terms of the contribution of final consonant duration to the estimated ISI means.

Since the estimated ISI values have been calculated on the basis of responses collapsed over subjects within each of the two language groups, they cannot be easily used to determine differences between the two groups. To examine such differences, the number of 'late' responses for each subject for each stimulus token were calculated--that is, the responses were collapsed over the nine experiment ISI durations. The more 'late' responses a token receives, overall, the earlier in the token the P-center occurs. To balance the number of subjects within each language group the responses from two Japanese subjects were not included. The two subjects chosen had participated in a rhythmic production test (utilizing Japanese stimuli only) prior to the perceptual test. These responses were then submitted to a mixed-design, repeated-measures analysis of variance (ANOVA) with the factors Stimulus Token and Language [3]. The cell means for number of 'late' responses in each language for each stimulus token appear in Table 2. The ANOVA showed significant main effects of both Stimulus Token ($F(10,280)=21.08$, $p<.001$) and Language ($F(1,28)=4.68$, $p<.05$). In addition, there was a significant Stimulus Token x Language interaction ($F(10,280)=2.21$, $p<.05$).

First and as expected, these results demonstrate that the number of 'late' responses given to a stimulus token seems to vary as a function

of its final consonant/medial vowel durations. Second, these results show that there is a slight difference in the mean number of 'late' responses overall between the two language groups. Third, these results show that the two language groups tend to have a different pattern of 'late' responses across different stimulus. The difference is small, but with this number of subjects, significant. This difference is very likely related to the differential effect of final consonant duration on the perception of isochronous sequences in the two language groups. It is tempting to speculate that the response differences between the two language groups are related to the differences between English and Japanese in phonetically acceptable syllable structures--particularly with regard to syllable-final consonants. However, such speculation would obscure the more interesting discovery that the perceptual responses of English and Japanese subjects are very similar, despite phonological-phonetic and/or timing differences between the languages.

Table 2. Cell means for number of 'late' responses by language groups and stimulus token.

Stimulus Token	Subject Group	
	English	Japanese
dah	4.41	3.55
dodd	4.07	3.72
dot	5.90	6.24
dozz	4.41	3.76
doss	4.69	5.00
dosh	4.41	5.10
dotch	5.97	5.79
dodge	4.59	4.59
don	4.03	3.10
doll	3.48	3.31
dar	4.31	2.90

In summary, the data support the hypothesis by Hoequist (1983a) that the P-center effect is a universal phenomenon. In both groups the estimated value of the ISI between the third and fourth tokens required to produce an isochronous sequence was significantly related to the vowel duration of the fourth token. These data also show that there are some differences between the perceptual responses of the American English group and the Japanese group. In particular, the P-center locations estimated for the Japanese subjects do not seem to have been significantly affected by final consonant duration; only medial vowel duration. The P-center locations for the American English subjects were significantly affected by medial vowel duration, and additionally affected by final consonant duration at at least a marginally significant level.

The results presented here complement those presented by Hoequist (1983) and support the contention that the P-center phenomenon might be found in speakers/hearers of all languages, but many questions remain.

How might the P-center effect operate in the production or perception of Japanese stimuli having either a light (one-mora) or a heavy (two-mora) syllable. Is the P-center related only to single syllable production/perception or does it also relate to a language's more global rhythmic organization? It thus goes without saying that much work remains to be done in understanding the organization of timing in both the perception and timing of speech. However, in this volume dedicated to Ilse Lehiste, we should take the space to briefly acknowledge the numerous contributions which Ilse has made to field in the areas of speech timing and prosodic phenomena in particular, and to the understanding of linguistic phenomena in general. Ilse has provided many important experimental and theoretical contributions, of course, but an even greater contribution is her insistence upon scientific rigor in the study of language behavior. She continues to provide our field with an example of the fertile scientific mind at work, and remains a scholar who is warmly appreciated by her colleagues who will value their less frequent interactions with her after her retirement.

Notes

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1. Morton et al. (1975) used spoken digits as their stimuli, and their precise definition of P-center more properly refers to the moment of occurrence of a spoken digit.

2. The terms P-center and stress beat, as used in the relevant experimental literature, seem to refer to the same linguistic phenomenon and the reader should assume that these terms are interchangeable in this paper.

3. Technically this analysis may violate one assumption underlying the use of parametric statistic analytic techniques. In particular, use of ANOVA assumes that the data analyzed are interval in nature. One could argue that since the calculated responses can only assume the values from 0-9, that they better represent ordinal level data. This type of violation is probably not very significant and actually is actually relatively common in psychological research. However, following Hays (1973), I will here caution that the ANOVA results may not accurately reflect the magnitude of the differences between language and/or stimulus tokens, but should tell us something about the quality differences between them.

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Ordering Paradoxes and Lexical Phonology *

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1. *Introduction*

In this paper I consider a rule ordering paradox in Kimatuumbi, a Bantu language of Tanzania. The paradox in Kimatuumbi concerns two rules, Glide Formation and Lengthening, and their relative orderings at different lexical levels. The paradox is that at level 2 the rule Glide Formation precedes Lengthening, but at level 3 Glide Formation follows Lengthening. The violation of the antisymmetry of rule ordering is only apparent, and results simply from viewing ordering relations between rules as conditions on the entire set of rules in a grammar. There is no paradox if rule ordering relations are a function of a particular phonological level in the sense of the theory of lexical phonology.

I also suggest more generally that properties of rules are susceptible to change at different lexical levels. The properties subject to cross-stratal changes might be rule-ordering statements as in Kimatuumbi, or changes in the class of input segments, or a change from obligatory to optional application. The proposal that the form of a rule is not entirely fixed might be implemented by viewing the core of a rule as being in a sense underspecified, and having missing properties filled in at particular levels. One of the properties a rule will have which is tied to particular levels even in the current conception of grammatical organisation in lexical phonology is a specification of the levels at which a rule applies.

As a preliminary to arguing for cross-level reordering in Kimatuumbi, I will briefly consider the issue of changes in the properties of rules between lexical levels. There are various cases in the literature where two formally uncollapsible rules are, according to at least some people's theoretical intuitions, one rule. Mohanan (1982) discusses two rules in Malayalam, a- deletion illustrated in (1) and nasal-deletion, illustrated in (2).

(1) a → ∅ / ___ [C *a-deletion* (Level 1)

aarogyam	'health'	an-aarogyam	'ill health'
aikyam	'unity'	an-aikyam	'disunity'
sukham	'happiness'	a-sukham	'unhappiness'
kramam	'order'	a-kramam	'disorder'

(2) nasal → ∅ / ___ [*nasal-deletion* (Level 2, 3)

[vrksam] [agram] → [vrakshagram]	'tree top'	(level 2)
[maram] [kutira] → [mara(k)kutira]	'wooden horse'	(level 2)
[sukham] [dukkham] → [sukhadukkham]	'pleasure and pain'	(level 3)
[sukham] [asukham] → [sukhaasukham]	'happiness and sorrow'	(level 3)

A number of dissimilarities between the two rules prevent them from being collapsed into one rule. The rule a-deletion only applies to a, only applies at level 1, and only applies before consonants. The more general nasal deletion rule applies at levels 2 and 3, applies to all nasals, and applies whether a consonant follows or a vowel follows. With the usual assumptions about rule writing, these differences are sufficient to prohibit (1) and (2) from being collapsed. However the rule applying at levels 2 and 3 is essentially identical to the earlier rule, with certain focal and environmental restrictions being dropped. Ignoring the question of a formal notation for level-dependent conditions on rules, a unified rule of nasal deletion in Malayalam might be written as in (3).

(3) $\langle$+nasal$\rangle \rightarrow \emptyset$ / ____] $\langle$C$\rangle$ $\langle$+coronal$\rangle$

Level 1: conditions present
Level 2, 3: not present

The meaning of the angled brackets and level-conditions is simply that at level 1, the conditions on the rule enclosed in angled brackets must be satisfied, while at levels 2 and 3, the conditions are dropped. The reason why such a collapsing is not so immediately obvious is that neither of the two independent rules of nasal deletion is tremendously complex or unnatural, so there is not an overwhelming sense that a major generalisation has been lost by having two unrelated nasal deletion rules.

Another case of phonological rules exhibiting changing properties at different phonological levels is Shona, which has a number of tone rules which are functionally similar but which cannot be collapsed into a single rule due to differences in morphological rule domain or minor differences in the conditioning environment, or due to ordering restrictions. In the analysis of Odden (1981), Shona has a number of H tone lowering rules, given in (4).

(4) *Rhythm*: H → L / H ___ H
[+prefix]

Associative Lowering: H → L / H ___
[+assoc.]

Clitic Lowering: H → L / H $\emptyset$ ___

Sandhi Lowering: H → L / H ($\emptyset\emptyset$) ___ $\emptyset\emptyset$ H

The common element in all of these rules is simply 'H lowers after H', with additional phonological and morphological conditions being imposed on different manifestations of the rule. Each of the rules in (4) applies at a particular lexical or postlexical domain, as indicated diacritically by the use of boundaries and morphological features. The tonal grammar of Shona could be streamlined by treating some or all of these putatively separate lowering rules not as different rules but as the same rule, with different conditions imposed at various lexical strata. Similar analyses may allow the unification of the family of Greek vowel-deletion rules discussed by Kaisse (1985), or handle the level-determined conditions on a-raising rule in Sekani discussed in Hargus (1985).

2. *Kimatuumbi Phonology*

Let us now turn to the argument for level-determined changes in rule order. First some information about the morphology. Nouns appear in one of 17 classes, a sample of each class seen in (5).

(5)	Class	Noun	Stem	Gloss	Prefix
	1	mwaána	-ana	child	mu-
	2	baána	-ana	children	ba-
	3	nkʉn'ʉundo	-kʉn'ʉundo	sieve	mu-
	4	mjkʉn'ʉundo	-kʉn'ʉundo	sieves	mi-
	5	ljkʉn'ʉʉnda	-kʉn'ʉʉnda	filtered beer	lj-
	6	makʉn'ʉʉnda	-kʉn'ʉʉnda	filtered beers	ma-
	7	kjgómá	-gómá	cassava (sg)	kj-
	8	jgómá	-gómá	cassavas (sg)	j-
	9	changaláwe	-changaláwe	gravel (sg)	∅
	10	changaláwe	-changaláwe	gravel (pl)	∅
	11	lugój	-goj	rope	lu-
	12	kalaáj	-laáj	little bamboo	ka-
	13	tulaáj	-laáj	little bamboos	tu-
	14	bweémbe	-eémbe	flour	bu-
	16	pakjgómá	-(kj)gómá	at the cassava	pa-
	17	kukjgómá	-(kj)gómá	to the cassava	ku-
	18	mukjgómá	-(kj)gómá	in the cassava	mu-

My assumptions about the morphology of these noun class prefixes are the following. At level 1, most of the lexical noun class prefixes are affixed to stems -- most, except the class 5 prefix lj-. At level 2 the remaining lexical prefix lj- is affixed, and at level 3, the locative prefixes are affixed. The assignment of locative prefixes to level 3 is well motivated on morphosyntactic and phonological grounds. The assumption that affixation of the class 5 prefix lj- occurs at level 2 explains a number of anomalous phonological properties of this prefix, which acts as though it is not present for a number of level 1 rules. Such rules include Post-Prefix H Tone Assignment (PPHTA) and Accent Erasure. As seen in (6) PPHTA assigns a H to the first stem vowel of a noun after a level 1 prefix such as ma, ka or lu; hence PPHTA applies after most lexical class prefixes, but fails to apply after locative prefixes and also fails to apply after the level 2 prefix lj- (for further information about Kimatuumbi tone, see Odden (1982) and Kisseberth and Odden (1980)).

(6)				
	ɑ́ɲɲaáj	'hospital' (Cl. 9)	ma-ɑ́ɲɲaáj	'hospitals' (Cl. 6)
	mu-ɑ́ɲɲaáj	'in the hospital'	ka-ɑ́ɲɲaáj	'small hospital' (Cl. 12)
	lj-ɑ́ɲɲaáj	'huge hospital' (Cl.5)	tu-ɑ́ɲɲaáj	'small hospitals' (Cl. 13)

$$\emptyset \rightarrow H \;/\; \underline{\qquad} \qquad \textit{Post Prefix H Tone Assignment} \text{ (Level 1)}$$

$$V + \overset{\vdots}{V} \quad V$$

A second phonological argument for excluding lj- from the set of regular noun class prefixes is the fact that it undergoes Lengthening, one of the two rules which forms part of the ordering paradox. Regular noun class prefixes do not undergo Lengthening.

2.2. *Glide Formation*

Let us now consider the rules involved in the paradox, beginning with Glide Formation. As the data in (7) show, a prevocalic high vowel becomes a glide, with compensatory lengthening of the following vowel.

(7)	/ɳ-ɗanɟɸ/	→ ɳɗaanɟɸ	'firewood piece' (Class 11)
	/kɟ-ɠlɨ/	→ kyuɠlɨ	'frog' (Class 7)
	/ĩ-ɛɛke/	→ iyɛɛke	'storage structure' (Class 5)
	/mɳ-ɟ-ɠɛɛkɾo/	→ mwɟɟɛɛkɾo	'in the cooking pots' (locative)
	/mɳ-ɛanɟɸ/	→ mwaanɟɸ	'in the firewood' (locative)

This Glide Formation rule applies to the vowel of lexical noun class prefixes, to the level 2 prefix ɟɸ-, and the locative prefixes. The high vowel of a verbal subject prefix or object prefix also undergoes Glide Formation.

(8)	/tɳ-cónde/	→ twcónde	'we should peel' (tɳ = 1p Sub)
	/a-ɟ-cónde/	→ aycónde	'he should peel them' (ɟ = Cl. 9 Obj)
	/kɳ-kɟ-épɳa/	→ kɳ-kyɛépɳa	'to avoid it' (kɟ = Cl. 7 Obj)

Glide Formation is formulated in (9), to detach a high vowel from the syllable nucleus, leaving behind a stranded V node, which results in compensatory lengthening of the following vowel.

(9) V V *Glide Formation*
 ╪
 [+high]

There are a few restrictions on (9) to be explained. First, Glide Formation does not apply between words.

(10)	ɟiɨatɸ jɠɳumbwɟiɨe	'the shoes fell'	*iɨaatw iitumbwiike
	lwiɑnɟɸ alwó	'that firewood'	*lwiɑnjw ɑalwó

This restriction indicates that Glide Formation is a lexical rule. The alternative would be to assume that Glide Formation is postlexical, but is ad hoc restricted not to apply between words. This alternative can be ruled out directly, by considering the remaining conditions on Glide Formation.

The second condition on Glide Formation is that it applies from left to right, and can not apply to a long vowel. These conditions are illustrated in (11), where the infinitive prefix kɳ- precedes the object prefix ɟ-, which precedes the vowel-initial verb -ɛɛrwa.

(11) /kɳ-ɟ-ɛɛrwa/ → kwɟiɛɛrwa 'to understand it' (kɳ = Infin, ɟ = Cl. 9 Obj)

The infinitive prefix vowel glides, compensatorily lengthening the following vowel. The derived length on the prefix ɟ then prevents that prefix from gliding. The prohibition against long vowels undergoing glide formation is an instance of the Linking Constraint (Hayes (1986)), which states that a rule which mentions the linking of the CV tier and the segmental tier must interpret those links as exhaustive. Since the prevocalic long vowel ii in (12) is linked to two V's, not one, long vowels do not satisfy the structural description of Glide Formation.

(12)

Right-to-left application of Glide Formation would yield *kɥjeliɕwa.

A third condition on Glide Formation is that if any syllable precedes the focal high vowel, Glide Formation is optional. When preceded by the syllable of the infinitive prefix in (13), object prefixes optionally undergo Glide Formation. In contrast, word-initial prefixes must undergo the rule.

(13)	kɥ-ki-ɬandjka →	(opt)	kɥkyɬandjka	'to write it'
	*ki-ɬandjka →	(oblig)	kyɬandjka	'to write it'
	naa-i-ɓewɲje →	(opt)	naayeɓewɲje	'I understood it'

Now we turn to two arguments that Glide Formation has cyclic behavior, and is thus not postlexical. Recall from (11) that in a string of level 1 prefixes, Glide Formation applies left-to-right. Consider the forms in (14) with a locative prefix followed by vowel initial noun class prefix, followed by vowel initial noun root.

(14)	[mɥ [i-ɬlà]] →	mɥyuɬlà	'in the frog'
	[kɥ [i-ɬɔ́]] →	kɥyɬɔ́	'to the cooking pots'

The vowels of the locative prefixes kɥ- and mɥ- and the noun class prefix i are all underlyingly prevocalic, at least in the traditional sense of underlying, yet in (14) the noun class prefix vowel undergoes Glide Formation, not the leftmost prefix vowel. How then do we explain the contrast in (15) between /mɥ-i-ɥɬ/ which becomes mɥjɥɬ, and /mɥ-i-ɬlà/ which becomes mɥɥɬlà?

(15)	a. [mɥ [i-ɬlà]]	→ mɥyuɬlà	'in the frog' (mɥ- = loc., i- = Class 8 noun)
	b. [mɥ-i-ɥɬ]	→ mɥjɥɬ	'you should pull it' (mɥ- = 2pl Subj., i- = Class 8 Obj.)

The relevant distinguishing feature is the difference in morphological structure. If Glide Formation is a lexical rule applying at levels 1 through 3, then we would expect a pattern of cyclic behavior, exactly as we have here. At level 1, Glide Formation applies in (15 a.) to the only prevocalic high vowel, the class prefix vowel i-, and in (15 b.) the rule applies to the leftmost prevocalic high vowel, the vowel ɥ- of the subject prefix mɥ-. If at a later level a locative prefix is added as it is in (15 a.), then Glide Formation might in principle be reapplicable, -- but in the present instance, Glide Formation fails to apply to the locative prefix since it is not followed by a vowel at that level of the derivation. All we need to assume is that Glide Formation applies at multiple levels, in order to get this cyclic effect. If Glide Formation is postlexical, then it should be blind to the difference between level 1 prefixes and level 3 prefixes, and all vowel sequences would incorrectly be treated alike. Therefore, Glide Formation must be lexical.

There is a second argument for the cyclicity and lexicality of Glide Formation. Recall from (13) that Glide Formation was optional when preceded by any syllable in the word. But the data in (16) run afoul of that generalization, in that the medial syllables kɥ- et al. must undergo the rule.

(16)	¶ pa [kɥ-ɬan-jɨ]] →	(ɬáŋ)	paɓwɬanjɨ	'at the firewood'
	¶ mɥ [kɨ-ɬɨ]] →	(ɬáŋ)	mɥkyɬɨ	'on the family farm'

If Glide Formation applies from inner levels out (i.e. is lexical), then the predicted results are the actual results.

(17) a. [lɨ-ɗanjó] *Output from level 1 morphology*
[lwɨanjó] *Glide Formation* (oblig)
[pa lwɨanjó] *Output from level 3 morphology*
'at firewood'

b. [a-lɨ-ɗandiiɨke] *Output from level 1 morphology*
[a-lɨ-ɗandiɨke] *Glide Formation* (optional; not applied)
'he wrote it'

The prefix *lɨ-* is encountered at level 1, and is not preceded by any syllable at that level. Glide Formation must apply, since the condition which allows optional application of Glide Formation is not present. At level 3 a locative prefix is added to the noun, but Glide Formation was previously required to apply at level 1. In contrast, when the high vowel prefix is preceded by another syllable at its own level, as (17 b.), then the prefix *lɨ-* undergoes Glide Formation optionally.

In the two preceding arguments for the lexicality of Glide Formation, a pattern of cycle-like behavior was encountered. In the theory of lexical phonology, there are two sources of cycle-like behavior. If a particular level is a cyclic level, then the output of each morphological affixation is submitted to the phonology, hence each morphological process constitutes a cyclic domain. The second source of cyclic behavior is the interaction of levels: a rule found at two levels will exhibit cyclic behavior with respect to the domain defined by the morphology of different levels. The cyclic behavior found in Kimatuumbi is of the latter type, since it cannot be of the former type. Specifically, if level 1 were a cyclic level, sequences of prefixes affixed at the same level should exhibit the same type of cyclic pattern as sequences of prefixes affixed at different levels. This is not the case, as the contrasts in (16) and (17) show.

2.3. *Lengthening*

Now we turn to Lengthening. The lengthening rule is a bit peculiar, in that it is not a strikingly phonetically motivated rule. The rule lengthens any vowel in a level 2 or level 3 prefix which stands before a disyllabic noun stem with short nuclei. The data in (18) show the lengthening of underlying short vowels of the level 2 class 5 prefix *lɨ-* and the level 3 locative prefixes *mɨ-*, *pa-* and *ku-* before such nouns.

(18) mɨɨ-chɸpa	'in the bottle'	paa-chɸpa	'at the bottle'
liɨ-chɸpa	'huge bottle'	kuu-ngó	'to the ropes'

I assume the formulation of Lengthening given in (19).

(19) V → VV / n [n n +noun] *Lengthening* (L2–postlexical)
| | | +stem
_ V V

Stems with 3 or more syllables or stems with long vowels do not condition Lengthening.

(20) mɨ-mbáingo 'in the cave' pa-changaláwe 'at the grave'

Lengthening also operates as a sandhi rule between words.

(21) /naammwénj pfli/ → naammwénjj pfli 'I saw the puff adder'
/balyu mbuyá/ → balyuu mbuyá 'that is grandmother'

Lengthening must be both lexical and postlexical, and when it applies lexically, it applies at levels 2 and 3. Lengthening does not apply to the level 1 noun class prefixes, as seen in (22). There is nothing about the phonological structure of level 1 prefixes which prevents them from lengthening -- it is simply the fact that Lengthening does not apply at level 1.

(22) kj-ílbe → *kjjílbe 'thing' (Cl. 7)
j-gómá → *jjgómá 'cassavas' (Cl. 8)

I note in passing that the failure of Lengthening to apply at level 1 refutes the Strong Domain Hypothesis (Kiparsky (1984)), which states that a grammar may only stipulate where a rule ceases to apply, and that a rule is always potentially applicable at level 1. A similar counterexample to the Strong Domain Hypothesis was presented in Hargus (1984).

As a further restriction on Lengthening, the rule does not apply to any prefix or word before CVCV adjectives, hence the restriction in (19) to nouns.

(23) mu-njín[→ *muunjín['in the small (x)'
aatwétj njín[→ *atwétjj njín['he took the small (x)'

Lastly, Lengthening does not apply before disyllabic nouns which are composed of a CV class prefix and a CV stem. Thus, Lengthening applies only before a disyllabic stem, hence the restriction in (19) to stems.

(24) mu-kj-kó → *muukjkó 'in the navel' (Cl. 7)

2.4. *The Paradox*

Finally we come to the ordering of Glide Formation and Lengthening, and the ordering paradox. Glide Formation and Lengthening necessarily conflict; Glide Formation cannot apply to long vowels, and Lengthening does not apply to glides. Looking at the forms in (25) where both Glide Formation and Lengthening could apply, we see that when the level 2 prefix lj- precedes a VCV stem, Glide Formation wins out over Lengthening.

(25) [lj [owá]] → lyoowá 'beehive' (Class 5)
[lj [até]] → lyaaté 'huge banana hand' (Class 5)

Had Lengthing applied first, Glide Formation could not have applied, since long vowels can not glide, and we would have derived incorrect *ljaaté. Therefore Glide Formation precedes Lengthening, at least at level 2. Now consider the interaction of Glide Formation and Lengthening at level 3. Here the paradox surfaces. The data in (26) show that when the locative prefixes ky- and mu- precede a vowel-initial disyllabic noun, one with no noun-class prefix such as até, then Lengthening wins out over Glide Formation.

(26) [mu [até]] → muuté 'in the banana hands'
[mu [gó]] → muugó 'in the gizzard'

If Glide Formation applied before Lengthening in (26) incorrect forms like ***mwaali* would be generated. Thus Lengthening precedes Glide Formation. But we have also seen that Glide Formation precedes Lengthening in the case of the level 2 prefix *li-*.

It is apparent that some type of ordering paradox is at hand; Glide Formation must precede Lengthening, but it must also follow Lengthening. Yet there is no paradox at all, that is no violation of the assumption of antisymmetry in rule ordering if we modify our conception of the way ordering statements are built into the phonology of a language. If instead of being a property of the phonology as a whole, we assume that the order of a rule is a property of the phonological level to which the rule belongs, then just as we have to say that the level 1 phonology contains the rule Glide Formation and not Lengthening, we also say that the level 2 ordering of Glide Formation and Lengthening is Glide Formation before Lengthening, and the level 3 ordering of these two rules is Lengthening before Glide Formation.

Notes

* An earlier version of this paper was presented at the 1985 Winter LSA Meeting in Seattle. I would like to thank Emmanuel Manday, from whom these data were collected. The transcription of Kimatuumbi is straightforward, except that n' represents *ŋ* and *ị* and *ụ* represent high tense vowels, which contrast with mid tense vowels transcribed as *i* and *u* and with mid/low lax vowels transcribed as *ɛ* and *ɔ*.

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The Slovenian Orphan Accusative, Component Interfaces, And Covert Grammatical Categories

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1. The Slovenian Orphan Accusative

Perlmutter and Orešnik (1973; hereafter P&O) observe that Slovenian exhibits all of the syntactic phenomena in (1) through (5), and they propose that these generalizations are nearly sufficient to explain the appearance in the language of a surprising construction they call the Orphan Accusative (OrphACC). The additional assumption needed to predict the OrphACC, in P&O's account, is rule ordering. In the remainder of this section I will illustrate the OrphACC and sketch P&O's analysis, which is couched in transformational terms. In the next section I observe that this analysis has several unfortunate properties, but that they vanish when the analysis is recast in nontransformational terms. However, the involvement of the grammatical feature of animacy in these phenomena turns out to be problematic. In sections 3-5 I shift from Slovenian to Russian and discuss the analytical and theoretical issues that arise there from the interactions of case, gender, number, and animacy.

(1) The ACC form of the MASC SG is identical to the GEN form for +AN (animate) Ns, to the NOM form for -AN (inanimate) Ns; FEM SG Ns have distinct NOM, ACC, and GEN forms.

(2) Modifiers - in particular, adjectives and determiners - agree with their head Ns in GEND, CASE, and NUM.

(3) A definite pronoun can serve as an NP marking identity of sense as well as identity of reference.

(4) A definite pronoun cannot serve as a modified N marking identity of sense, however; instead the N slot is empty when there are modifiers.

(5) All definite pronouns, regardless of their reference, are grammatically +AN; in this respect they are like certain referentially inanimate nouns that are grammatically +AN, like *as* 'ace'.

Consider what happens when we construct a NP containing both an adjectival modifier and an identity-of-sense anaphor referring back to some earlier ACC SG N. According to (3) the anaphor can be a definite pronoun, and according to (5) such a pronoun will be +AN, but according to (4) it will not be realized phonologically. As for the modifier, what the remaining principles, (1) and (2), predict will depend on the GEND and AN values of the pronoun. If the pronoun is FEM, then (1) says it has a distinct ACC SG form, and (2) says that the modifier has the agreeing features CASE:ACC, GEND:FEM, and NUM:SG; these predictions are verified in (6c).

(6) -AN FEM ajda 'buckwheat':

a. Katero ajdo hočete? 'Which buckwheat do you want?'
b. Hočem navadno ajdo. 'I want ordinary buckwheat.'
c. Hočem navadno. 'I want ordinary.'

If the pronoun is MASC, however, then (1) says that (since the pronoun is +AN, even for an inanimate referent) its form is GEN SG, and (2) says that the modifier has the agreeing features CASE:GEN, GEND:MASC, and NUM:SG; these predictions are verified in (7d), which contrasts with the ungrammatical (7c) - though (7c) is what we would expect from simpleamindedly solving the analogical equation (6b) : (6c) = (7b) : X.

(7) -AN MASC ječmen 'barley':

a. Kateri ječmen hočete? 'Which barley do you want?'
b. Hočem navaden ječmen. 'I want ordinary barley.'
c. *Hočem navaden. 'I want ordinary (=NOM).'
d. Hočem navadnega. 'I want ordinary (=GEN).'

It is the form in (7d) that P&O identify as the OrphACC: a MASC (but not FEM) SG modifier in the ACC (but not any other) case which is 'orphaned' - that is, which is in combination with an empty N - and so has a special form, identical to the GEN. P&O's account of the OrphACC, which I have sketched informally above, depends not only on having the principles (1)-(5) in Slovenian, but also on several assumptions about the interactions among these principles, assumptions that were only implicit in my sketch. P&O, however, are quite explicit about these interactions. They assume three ordered transformations, which I paraphrase in (8): Pronominalization, corresponding to principle (3) but also incorporating a call on the lexicon, where P&O apparently assume principles (1), ACC Prediction, and (5), Animacy Prediction, apply; Agreement, corresponding to principle (2); and Pronoun Deletion, corresponding to principle (4). That is, pronouns are introduced as replacements for nominal constituents, and Agreement is determined with respect to these pronouns rather than the NPs they replace; having done their work with respect to Agreement, the pronouns are then deleted.

(8) Pronominalization. A nominal constituent identical in sense to an antecedent constituent is replaced by a definite pronoun.
Agreement. A modifier agrees with its sister nominal constituent.
Pronoun Deletion. A definite pronoun is deleted when it is modified.

2. Component interfaces I

P&O's analysis predicts the OrphACC very nicely, but it has four aspects that are, to my mind at least, unsatisfactory. First, it seems to be intractably transformational; a nontransformational alternative is to be preferred if at all possible. Second, it posits a rule replacing anaphoric full NPs by pronouns, a step that is not easy to motivate even in transformational frameworks. Third, it relies on parochial (that is, language-particular) rule ordering; interactions predicted on universal principles are to be preferred wherever possible. Fourth, these parochial rule orderings include the stipulation that lexical insertion precedes Agreement: 'We are now proposing that the Orphan Accusative arises from the application of the rule of Concord at the stage of derivations at which the underlying head noun has been replaced by a pronoun.' (P&O: 427)

Ordering lexical insertion before the syntactic rule of Agreement is a particularly bad move, since lexical insertion (at least as P&O seem to understand it) makes available the full set of properties of lexical items: the values of features like AN, the choice of declensional paradigm, presumably even the constituent morphemes within the item and its phonological properties. That is, this part of the analysis makes it impossible to maintain sharp interfaces between the components of syntax, morphology, and phonology; but see Zwicky and Pullum (1986) and references therein for arguments that the autonomy of components should be maintained if at all possible. If the component boundary can be breached in this instance, then what sorts of interactions between syntax on the one hand and morphology and phonology on the other are excluded?

Fortunately, P&O's analysis of Slovenian can be translated into one that is free of the unsatisfactory aspects of the original - indeed, one that is fully consistent with the phrase structure framework of generalized phrase structure grammar (GPSG; see Gazdar et al. 1985). In such a framework there is no rule of Pronominalization; rather, pronouns are distributed freely in syntactic structures, subject only to local restrictions on their occurrence (and of course to a nonsyntactic requirement, that they must be semantically interpretable). Among the pronouns of Slovenian is an empty N, which I will assume has the features N[+PRO, +DEF, +NULL]. This is no analytic innovation, since empty constituents of several types are now assumed in virtually all frameworks for syntactic description, including GPSG. There is then no Pronoun Deletion rule, but only principles distributing values of the feature NULL within branchings; one such principle disallows nominal constructs consisting of a [-NULL] modifier and a [+NULL] head.

The two aspects of their analysis that P&O treat as specifically lexical - ACC Prediction and Animacy Prediction - will be treated instead as syntactic principles, determining the values of CASE and AN, respectively, within a category on the basis of other features in that category (as Feature Co-occurrence Restrictions or Feature Specification Defaults, in the terminology of Gazdar et al. 1985). In particular, Animacy Prediction will require that an N with the features [+PRO, +DEF] also has the feature [+AN].

3. Covert grammatical categories I

The Slovenian analysis is still not trouble-free, however, since a family of problems surrounds the formulation of ACC Prediction. Thus far I have provided only informal characterizations of this principle, characterizations in which the FEM ACC, the MASC 'animate ACC' that is identical in form to the GEN, and the MASC 'inanimate ACC' that is identical in form to the NOM are systematically treated *both* as instances of a single grammatical category (ACC) *and also* as instances of three distinct grammatical categories (ACC, GEN, NOM). I will argue that the correct analysis does, in effect, have it both ways, but it is clear that in a nontransformational framework we cannot literally assume that an 'animate accusative' has both the feature CASE:ACC and the feature CASE:GEN in its syntactic description, for that would be contradictory. I have elsewhere (in Zwicky 1986b) argued that multiple feature marking should be countenanced in syntactic theory - but for the purpose of distinguishing inherent features from those imposed by rules of agreement or government, or of distinguishing impositions arising from different sources, and I cannot see that these proposals are applicable in the instance at hand.

I will begin, then, by considering analyses that choose one or the other of these feature assignments in the syntax. My discussion will use data from

standard Russian rather than Slovenian (simply because I am more familiar with Russian), but the main points are common to most, if not all, of the modern Slavic languages.

First, however, some theoretical preliminaries. The feature AN is centrally involved in the discussion of sections 4 and 5. And it is important that AN is a *covert* grammatical category in Russian, like CT (count versus mass), HUM (human versus nonhuman), DEF, WH, and TR (transitive versus intransitive) in English. What these features share is a morphological property, the fact that they are not *inflectional*, in a technical sense of that word: no inflectional rules (of the sort in Zwicky 1985a) provide exponents for them. In this regard they are unlike overt grammatical categories (for instance, CASE and NUM in Russian and English). Covert categories are conveyed by wholesale distinctions between lexical items (the versus *a* in English) or sometimes by derivational morphology (as when derivation provides +TR verbs corresponding to -TRs, or vice versa), and of course they are distinguishable via their different cooccurrence possibilities (as when SG +CT Ns require an article in English while SG -CT Ns can occur without one). But no rule of inflectional morphology provides an exponent for a covert category.

Within the framework of GPSG, overt categories in a language are head features in that language, subject to the Head Feature Convention (HFC); that is, the default is for the head constituent of a construct and the construct itself to share their values for such features. Covert categories in a language, I should like to claim, are never head features (though they can be GPSG foot features); this restriction on the role of covert categories in a grammar is similar in spirit to the prohibition in Zwicky (1986b: sec 4.3, citing Cooper 1986) against having 'silent features' distributed by the HFC. In any event, one important consequence of the restriction is that covert categories cannot participate in grammatical agreement, since the Control Agreement Principle (CAP) of GPSG, which requires that certain sister constituents share their feature values, applies only to a subset of the head features in a language.

(I must stress here that which categories are overt and which covert is a parochial matter. Chinese has no overt categories at all; (sex) GEND is covert in English but overt in Russian and many other European languages; AN, HUM, and CT are covert in English and Russian but overt in Swahili and many other Bantu languages; and so on.)

But why should I want to exclude covert categories, like AN in Russian, from the set of head features and so exempt them from the HFC and the CAP? Because I hope to constrain the feature-manipulating mechanisms of GPSG. The CAP and HFC together can have the effect of 'spreading' feature values throughout trees, both horizontally and vertically, from one branching to another, whereas the Foot Feature Principle (the only comparable mechanism for foot features) is much more restricted in its effects, being essentially capable only of spreading a feature value down from the category in which it is introduced by rule.

Now the combined power of the HFC and CAP is demonstrably needed for standard examples of grammatical agreement (to link the head N of the subject to the head V of the predicate, for instance), but in the absence of compelling evidence this power should not be extended beyond its traditional domain, where only inflectional feature values - that is to say, overt categories - are spread. Otherwise, we predict the possibility of syntactic dependencies of all sorts between widely separated words; the appearance of a particular head N in the subject (say, kangaroo or sugar, but not penguin or

gelt) might require that the head V belong to a particular conjugational class (say, the class with *-en* past participles, so that *break* and *speak* would be permitted Vs, but not *lyag* or *sleep*). Such dependencies are *logically* possible, but I do not believe they occur.

4. Component interfaces II

On to the facts of Russian. The ACC case is standardly described as occurring in a number of distinct syntactic constructions in the language; the list in (9) is extracted from Maltzoff (1984: 64-9). I assume here, without argument, that the morphological feature of CASE is assigned in two steps, sketched in (10) and (11); (10) assigns the GR (grammatical relation) DO (direct object) as a default (other rules will assign other GRs in more specific contexts), and (11) assigns ACC as the default CASE for DOs (other rules will assign other cases, in particular GEN). Values of CASE are spread to modifiers as in (12).

(9) a. Direct objects of most Vs
b. Objects of many Ps, including several that govern ACC in motional senses, PREP in locational senses
c. Objects of the A *žal'* 'be sorry for'
d. Bare NP expressions of extent (in time, distance, price, weight)

(10) The default value of GR for an NP daughter of VP or PP is DO.

(11) The default value of CASE for NP[GR:DO] is ACC.

(12) The CAP (together with the HFC) requires that modifiers share the values of CASE, GEND, and NUM with their head Ns.

(13) a. The ACC MASC SG form = the GEN form for +AN Ns
b. the NOM form for -AN Ns
c. The ACC NEUT SG form = the NOM form
d. FEM SG Ns have distinct NOM, ACC, and GEN forms
e. The ACC PL form = the GEN form for +AN Ns
f. the NOM form for -AN Ns

The question is now how the ACC Prediction facts, summarized in (13), should be incorporated into a syntactic description of Russian. I begin with the approach outlined in (14), which takes quite literally the claims in (13) that particular forms are identical to one another and so uses, in (14b), a mechanism of morphological description - the rule of referral, developed in Zwicky (1985a, b) - rather than syntactic mechanisms beyond (10)-(12). On this analysis, the ACC SG modifiers *staroqo* and *staroe* in (13) have the forms they do *because* their head Ns have forms identical to the GEN and NOM, respectively.

(14) ACC Prediction is entirely a matter of morphological rules, which refer some realizations of ACC to NOM or GEN.
a. CASE:ACC is determined as in (11).
b. The realization of GEND:MASC and NUM:SG for CASE:ACC is referred to CASE:GEN for +AN Ns, to CASE:NOM for -A Ns.
c. Modifiers agree with the categories that are morphologically realized on their head Ns.

(15)

	+AN MASC 'old cat'	-AN MASC 'old table'
NOM	staríj kot	staroe stol
ACC	starogo kota	staroe stol
GEN	starogo kota	starogo stola

The analysis in (14) is a disaster from the theoretical point of view. Once again, the boundary between syntax and morphology would be breached. To get the right interaction between (14b) and (14c), with morphological realization preceding Agreement, either morphological realization must take place in the syntactic component, or Agreement must take place in the morphological component, or else the components as wholes must interact in exactly the opposite way from the one ordinarily assumed (in which syntactic rules are blind to the morphological composition of words, while morphological rules can be conditional on features distributed by syntactic rules).

Fortunately for component interfaces, (14) is simply wrong on factual grounds. There are clear instances of referral rules for Russian Ns, and in general these rules have no consequences whatsoever for the forms modifiers take. Thus FEM Ns ending in palatalized consonants have an ACC form that 'coincides with' the NOM (as Maltzoff (1984: 35) so carefully phrases it), but their modifiers nevertheless distinguish between ACC and NOM, as in the left column of (16). And MASC Ns ending in a have the declensional forms of the corresponding FEMs, including an ACC SG distinct from the NOM and GEN, but (as Klenin (1983: 9) observes) their modifiers nevertheless have syncretic realization, as in the right column of (16). It is also true that indeclinable Ns nevertheless have modifiers with full sets of declensional forms (as in the middle column of (16)), rather than an invariable form, as (15) would lead us to expect.

(16)

	FEM 'old mother'	+AN MASC 'old attaché'	+AN MASC 'old uncle'
NOM	staraja mat'	starij attaše	starij djadja
ACC	staruju mat'	starogo attaše	starogo djadju
GEN	staroj materi	starogo attaše	starogo djadi

A variant of the analysis in (14) that requires no extraordinary component interfaces can be framed along the lines in (17). This approach allows a description of the facts in the first two columns of (16) - mat' can have the value NOM (when its GR is SU) or the value ACC (when its GR is DO) in the syntax, and attaše can have the full range of CASE values in the syntax - but it founders on the right column, since a DO djadj- must receive the value ACC (so that its morphological realization can be distinct from the NOM and GEN) while its modifiers must receive the value GEN (because of *their* morphological realizations), thus contradicting the requirements of Agreement.

(17) ACC Prediction is managed by syntactic rules distributing the values NOM, ACC, and GEN for CASE for GR:DO NPs.
   a. As in (11), except that some Ns (according to their values of GEND and AN) require the values NOM or GEN for CASE, rather than ACC.
   b. As in (12).
   c. Morphological forms are chosen on the basis of the values of CASE.

5. Correct grammatical categories II

I conclude that the correct account of ACC Prediction in Russian is more abstract than the ones in (14) and (17), which embody versions of the claim

that the CASE you see is the CASE you get. Consider instead the approach in (18), which uses a (more morphological) feature DECL distinct from a (more syntactic) feature CASE. Forms like *stercgo dziqju* are no problem in this sort of analysis. Both head and modifier are CASE:ACC and (because the N is GEND:MASC and +AN) DECL:2. The N *dzqj-* belongs to a morphologically exceptional subclass of Ns whose declensional forms are referred to the FEM, while the A *star-* shows the default morphological forms for a word of DECL:2, including the referral of the ACC to the GEN. In a variant of this approach, outlined in (19), the feature AN is appealed to directly.

(18) ACC Prediction is managed by syntactic rules distributing a (purely morphological) feature DECL of declension class.
   a. As in (11), with other syntactic rules determining the values DECL:1/2/3 on Ns according to their values of GEND and AN.
   b. As in (12), except that modifiers also share the values of DECL on N.
   c. Morphological forms are chosen on the basis of the values of DECL.

(19) ACC Prediction is managed by syntactic rules distributing values of the (covert category) feature AN.
   a. As in (11).
   b. As in (12), except that modifiers also share the values of AN on N.
   c. Morphological forms are chosen on the basis of the values of AN.

From the theoretical point of view, both (18) and (19) are suspect, because they use the CAP and HFC to spread the *covert* categories DECL and AN, respectively - just the sort of use of noninflectional features that I spoke against in section 3. (Note that DECL, despite its name, is not inflectional in the technical sense; it conditions the choice of inflectional rules, but itself has no inflectional exponent.)

There are empirical problems as well, resulting from the fact that in these analyses genitive and animate accusative Ns do not constitute a natural syntactic class, but are related to one another only in the morphology. As it happens, however, there is at least one place in Russian syntax where [CASE:ACC, GEND:MASC, +AN] groups with [CASE:GEN] and the other *oblique* cases (DAT, PREP, INSTR), as against the *direct* cases [CASE:NOM], [CASE:ACC, GEND:NEUT], [CASE:ACC, GEND:FEM], and [CASE:ACC, GEND:MASC, -AN]: The cardinal number words 'two' through 'four' govern CASE:GEN and NUM:SG within NPs in direct cases, but within NPs in oblique cases they agree in CASE and NUM:PL with their heads (see Zwicky (1985b: sec 6.3) for a GPSG treatment of these and related facts). As a result, 'three cats' looks throughly PL (as well as genitive) in the ACC, while 'three tables' has a clearly SG head in the ACC, as in (20). But to state the generalization about CASE and NUM government with cardinal number words, we need to treat the syncretic ACCs that *look like* GENs as forming a class with the true GENs, which is not possible with the assignment of features used in (18) or (19).

(20)				
NOM	tri kota	'three cats'	tri stola	'three tables'
ACC	trjox kotov		tri stola	
GEN	trjox kotov		trjox stolov	

Clearly we need to have it both ways. In some ways MASC SG ACCs are distinct from GENs and NOMs, but in other ways the +AN ones are the same as GENs (and the -AN ones the same as NOMs). I propose to treat these cross-cuting assignments of forms to classes in the syntax as exactly parallel

to cross-cutting assignments of segments to classes in phonology. That is, I will decompose the values of the feature CASE into sets of features, thus splitting ACC into several subCASEs.

As a formal move, this has all the advantages of the analysis in (18) using the feature DECL, but does not involve spreading a covert category and permits the direct/oblique distinction to be made fairly simply (as in (23) below). The proposal is outlined in (21), and the roles played by the new features, X and Y, are specified by the rules in (22); note that (22c) says that the value of Y is closely related to, but not identical to, the value of AN. The rules in (22), together with the morphological defaults in (21c), correctly describe all of the facts about ACC Prediction listed earlier in (13).

(21) ACC Prediction is managed by syntactic rules determining the values of X and Y in CASE of N[CASE:(ACC)] according to the N's values of GEND and AN; see (22).
  a. The default value of CASE for NP[GR:DO] is (ACC); there are three subCASEs, (ACC, +X, -Y), (ACC, -X, +Y), and (ACC, -X, -Y).
  b. As in (12).
  c. Morphologically, the defaults are for the first of these subCASEs to be realized via the distinctly ACC forms, the second by *referral* to GEN, and the third by *referral* to NOM.

(22) a. If N is NUM:SG, GEND:NEUT, CASE:(ACC), then it is CASE:(-X, -Y).
  b. If N is NUM:SG, GEND:FEM, CASE:(ACC), then it is CASE:(+X, -Y).
  c. The default is for @AN, CASE:(ACC) N to be CASE:(-X, @Y).

(23) The direct CASEs are NOM and (ACC, -Y); all others are oblique.

The decomposition of CASEs into features, which plays such an important role in my analysis, is no cheap formal trick. Such a decomposition is called for in a large number of other instances. It is, I believe, the appropriate mechanism for stating that in Russian the prepositions alluded to in (9b) govern either ACC or PREP, depending on their meaning; syntactically, ACC and PREP should share a feature (call it +SPAT), so that the rule in question stipulates that objects of these prepositions are +SPAT, the objects of other prepositions being specified CASE:(+SPAT, +ACC) or CASE:(+SPAT, -ACC) or some other CASE entirely. Presumably, decomposition of CASE is also an appropriate method for dividing the CASEs of Russian into a direct and an oblique subset, +OBL being the default value of the feature in question.

Feature decomposition of CASE is also the natural way to describe the marginal or sporadic CASEs of many languages, for instance, PART and LOC in Russian and what I will call IGEN in English. Russian PART is a special set of forms used with partitive meaning, and it is available only for certain MASC nouns; otherwise GEN is used for partitives (Maltzoff 1984: 28f). Russian LOC is a special set of forms used with locational meaning, and it is available only for certain MASC nouns serving as objects of the two prepositions *v* and *na*; otherwise PREP is used for locationals (Maltzoff 1984: 30f). English IGEN is a special set of forms used with predicate possessives and possessive objects of the preposition *of* (*This book is mine. A book of mine*), and it is available only for the personal pronouns; otherwise GEN is used for possessives. In each such instance, we can say that the marginal CASE shares one feature with its default counterpart but differs from it on another

feature: CASE:PART = CASE:(+GEN, +PART), CASE:GEN = CASE:(+GEN, -PART), for instance. Then if rules for the default CASE are stated in terms of the shared feature they will cover the marginal CASE as well, unless there is a stipulation specifically to the contrary.

6. Concluding remarks

To sum up: My proposal treats what are sometimes, rather awkwardly, called the 'animate accusative' and 'inanimate accusative' of Russian, Slovenian, and other Slavic languages (as opposed to the plain 'accusative' exhibited by FEM SG Ns) as subCASEs of ACC, a move with parallels elsewhere in Russian and in many other languages. The analysis outlined in (21)-(23) then describes the facts of Russian without violating strong universal hypotheses about the interfacing of grammatical components and about the role of covert grammatical categories in syntactic rules.

One lesson to be drawn from this discussion is that we must insist as much as possible on having precise statements of grammatical rules, located within an explicit framework of assumptions. Truly formidable analytic problems, as well as central issues of theory, may lie concealed within informal statements like the Slovenian ACC Prediction rule in (1) or its more detailed Russian counterpart in (13). And traditional scholarship may give little hint of these complexities: 'It is a curious fact that questions of grammatical agreement which often baffle the non-native speaker tend to be treated in an offhand manner in Russian grammars and have not attracted much scholarly attention to date.' (Crockett 1976: 1)

Another lesson is that it is easy to underestimate the extent of grammaticization in particular languages, and indeed in Language. The first analyses I considered for Russian were attractive largely because they embodied the principle that the CASE you see is the CASE you get, a principle that directly reflects the central sound-meaning function of systems of agreement, according to which phonological identity signals grammatical relationship. It might be that systems of agreement arise, both diachronically and ontogenetically, to serve this function directly. But it seems that they become grammaticized, indeed syntactified, with lightning speed. Despite occasional appearances to the contrary, agreement systems do not seem to involve phonological or morphological copying, but instead are universally matters of syntactic feature sharing - a position that is in fact assumed without argument in the thoughtful crosslinguistic survey of agreement phenomena by Moravcsik (1978).

The evidence from Slavic suggests that fairly complex systems of grammatical agreement can be remarkably stable, once established through the side-effects of phonological change, through language contact, or whatever. I will not speculate on the historical origins of ACC Prediction in Slavic, a topic with a rich literature of its own. What is important here is that the outcome of these events is a synchronic system that might be to some degree marked but (like the other complex agreement systems discussed by Pullum (1984)) is nevertheless fully consistent with the requirements of universal grammar - which is to say that the system provides an excellent place in which to explore the consequences of particular theoretical hypotheses, such as those concerning component interfaces, covert grammatical categories, and the internal structure of syntactic features like CASE.

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Parameters and markedness in the acquisition of syntax.

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1. Four assumptions.

Assume to begin with that every element of a sentence must be accounted for (the 'full interpretation' of Chomsky 1984) or licensed, in acquisition as in the end-state grammar.

Assume secondly that a grammar is not a set of rules, but rather a set of just such licensing principles, again in acquisition as in the end-state. Some examples of principles in this sense are:

The Projection Principle	Theta Theory
X-Bar theory	Control theory
Government theory	Case theory
Binding theory	Subjacency.

Assume thirdly that all principles are, at least outside phylogeny, impenetrable, i.e., not influencable from the outside (cf. Pylyshyn 1980, the notion 'autonomy' in Chomsky generally, and its generalisation in Fodor 1983.

Assume finally that, although the principles are 'impenetrable' as such, certain of these principles have parameters of variation associated with them, again at each stage of acquisition.

Note that while a parameter might become frozen into a principle in the development of the species (the phylogenetic question), we do not suppose that a principle as such may become subject to variation; e.g., we don't expect variants of the Projection Principle or c-command to arise through historical change -- though see Section 4.4 for doubts.

Some examples of parameters are:

Order of Head-Complement structures
Adjacency-strength, for government
What qualifies as a proper governor wrt Extraction from Subject position?
Bounding Nodes, for Subjacency
Whether S' Pied-Pipes or not (German & English, vs.Dutch)
Whether INFL is in S (English) or VP (German)
Whether L_2 has an opaque or a transparent VP (for Theta-Binding)
Whether the R (the affix-hopping) rule applies optionally in the syntax, or only in the P-component.

Our principled goal is now that, if we activated a process called 'Do anything to (e.g., Insert, delete, coindex, substitute or adjoin) any constituent', the Principles and Parameter-settings should exclude all illicit output-sentences, language for language.

2. Types of Parameter.

2.1. The two kinds of parameter.

A parameter is a dimension of a principle for which overt evidence is available, and on which there might therefore be variation. And we expect a parameter to have some default (or, unmarked) value.

Yet some parameters (e.g., Head-Complement direction) have no default value. We assume that, unless indeed these prove to be complexes of more than one parameter, such parameters are 'open'; and the choice between alternative 'values' (rather than 'plus vs. minus') is language-specific.

Conversely, the principle behind a parameter itself is as I said 'impenetrable' in the sense intended, and can undergo no variation. Of course, the best way to view this distinction is in terms of 'natural laws' (for the principles) and 'conventions' (for the parameters).

2.2. Assigning parameters to their types.

We turn now to the question, which of the putative parameters of grammar are of which type, noting first that parameters naturally involve very diverse material: thus (e.g.,) some parameters delimit Categories (Categories for Wh-movement, the domain of V-max), others have to do with orientation (directionality of government or Theta-role assignment) or adjacency (strictness of adjacency for Case-assignment), and yet others have to do with rule-application levels (Wh-movement, Chomsky's 1981 A-rule). Take a few clear cases first.

Which parameters have default settings that may have to be adjusted? Candidates here are Bounding nodes for Subjacency ; here we might have an example of 'the value on a default parameter' being identified by a set, say NP and S (though cf below Sec.D.2.1). Also, there is adjacency for government/Case assignment (whose default value is 'strict' adjacency); and whether Prepositions govern structurally like verbs or not (here the default value is probably that they do not).

Candidates for truly 'open' parameters might be:
Directionality of Head+Complements
Which maximal projections undergo alpha-movement
Whether Wh-movement obtains in the syntax or only at LF
Whether the R-rule applies in the syntax or only at PF
Whether Lx has Subject clitics or not.

Conversely, to recapitulate, candidates for 'true universals' (our 'principles') are c-command, Subjacency as a principle of locality, the Binding principles, May's 1977 Q-rule for adjunction in LF, the Theta Criterion, and the Extended Projection Principle.

2.3. General on Triggers.

2.3.1. Definition.

The principles of grammar are absolute, as we saw; but their dependencies, the default and the open parameters, we defined as sensitive to

the environing language. A trigger in grammar development is a stimulus (group) (a) activating a schema (principle) or (b) setting the value /PA of a parameter. Under (b), a trigger thus allows for either the setting of an open parameter, or the changing or re-setting of a default parameter. Of course, these processes are covert and subsumable under Piercean 'abduction'.

2.3.2. Simplicity?

How simple (conversely, complex) can a trigger be? And on the other hand, how complex a pattern of elements-in-schemata or constraints can a single perhaps very simple trigger release?

In ethology, a trigger may be very simple; eg., in the case of birds the imprinting trigger is the first moving object seen after hatching. On the other hand, a complex sequence of stimuli and events must obtain before the triple-spined stickleback will lay her eggs.

So far as language development is concerned, Chomsky 1982 gives an example of what seems a very simple trigger. He says '..if children get information that something is a reciprocal then that ought to put into play a whole range of constraints as to whether and how it can be interpreted and construed'.

Notice that Chomsky is in effect defending a kind of a 'single-trigger' or 'unified onset' account of the activation of Binding Theory (the relevant constraint here) against Matthei's 1979 claim of piecemeal development. The whole Binding complex, then, depends for Chomsky on as simple a trigger as possible.

And we must perhaps talk, further, of indirect triggering: thus the presence of an otherwise unjustified Resumptive Pronoun in nursery-sentences such as:

'Who you you believe the story that he killed the dragon?'
'Who do you wonder why he killed the dragon?'

in serving to circumvent Subjacency, automatically also triggers the appropriate Bounding nodes.

2.4. Acquisition.

2.4.1. What is acquisition now?

In the present model, language-development does not consist of the cumulative acquisition of diverse rules of grammar, whether of Phrase-structure or Transformational. Rather, it consists very largely in the setting of just those interactive parameters across modules of the grammar. The process is largely data-driven, i.e., it takes place at least partly under the influence of the relevant environmental 'triggers', including heard and attended-to data.

Thus a given putative parameter either a) is 'open', so that a 'first guess' during acquisition may or may not be correct: if incorrect, the guess must be corrected, OR b) has a Universal (or, default) setting; in that case it must be reset only if disconfirmed, i.e., if the environment language has a marked (or, non-default) value for it, OR c) is not a parameter but a

principle; and a principle is simply a law.

2.4.2. Are all the stages of acquisition 'natural'?

It has been held (e.g., in White 1981) that, since the child never contravenes essential properties of language, his grammar will at every developmental stage represent a possible human language. Notice now that, if the above outline is valid, the presence of 'open parameters' perhaps does give early first language acquisition a unique status; to the extent that human languages do not seem to allow parameters to remain 'open', White is wrong. Earliest acquisition represents a partly unnatural language.

Note further that early first languages are also unnatural in a second respect; they are liable to cognitive constraints of a purely developmental kind (cf Rizzi on pro-drop acquisition, under 3.3.1.2. below).

3. Markedness in default parameters.

3.1. Unmarked as [+] or [-].

It is of course not the case that every UG principle has associated with it parameters that must be set [+] or [-] for each grammar. Furthermore, it is not the case that the default values of parameters are randomly assigned, as we seemed to imply above, in assuming that the unmarked value of a default parameter could be either [+] or [-].

Suppose we now assume, perhaps on grounds of economy, that the markedness of default parameters applies homogeneously, i.e., that all parameters have the same default value for the initial state of the acquisition device. There are now two possible scenarios, viz., the one with all Unmarked parameter values [+], the other with them all [-].

3.2. Homogeneous Unmarked values

Scenario 1. Suppose the Unmarked values are all [-]. Consider first the clear cases.

(a) [-] Preposition Stranding, since this implies that Prepositions govern structurally, as verbs do (Hornstein and Weinberg 1981) or that the language licenses reconstruction of V-P so that the Case & Theta-role assigning properties of P are transferred to V (Rouveret & Vergnaud 1980).

(b) [-] Presence of pronominal clitics

(c) [-] Subject pro-drop, thus allowing for the marked use in English Casual speech. This seems to hold equally of the interpretations a) the R-rule may obtain in syntax for pro-drop languages, or b) a head (INFL) may in Lx be licensed to give Case to NP-Subject position.

However, take now Subjacency as a more extended example. In the classical treatments (e.g., Chomsky 1981), there are two major components to Subjacency. The constraint to neighbourhood itself is presumably a law (in the sense here assumed). On the other hand, the so-called Bounding Nodes are parametrised; the possibilities ranging from S' thro S, NP, to PP (but not VP). English supposedly has S,NP as Bounding; while Italian has S' and NP, but

not S.

On the present interpretation, the learning model makes all the onset variables [-], so that none of the Categories is a Bounding Node in early acquisition.

Note that, by contrast to the supposition that the setting of Bounding nodes is achieved en bloc (the 'set' solution in Section 2.2.2. above), we will assume here that this setting applies to each Bounding node individually.

But in the absence of an auxiliary theory, it is impossible to reconcile the Bounding node settings with a default value of [-], because of the implausible implication that young children may freely violate Subjacency.

Scenario 2. Homogeneously, the Unmarked values are [+].

Consider now the case for [+] as the default value for all parameters. Notice the plausibility of this value wrt the problem of the Bounding nodes for Subjacency; for this value reasonably guarantees that no violations can occur. Positive evidence for a revision to a [-] value for, say, S as Bounding node would thus come primarily from the occurrence of sentences otherwise in violation of Subjacency wrt the node S, as in Italian.

But of course the homogeneous application of [+] as the default value in turn leads to contradictions; thus, assuming [+] for Preposition Stranding implies that, say, all French or German beginner language learners will produce such strandings, in fact illicit in their languages, and in fact unattested in early-acquisition studies for those languages.

3.3. 'Natural' default values.

3.3.1. The Subset Principle.

The 'homogeneous markedness' hypothesis having failed, we shall instead try to apply the learning-theoretic 'subset' principle, to the problem of defining markedness for default parameters. Conceptual parallels can indeed be found in the debate of the 70's on rule-ordering in phonology, viz., in work of Sanders (1970) and Koutsoudas, Sanders and Noll (1974). But the 'subset' principle is in its present form due to Berwick 1982 ; in effect, it says 'choose the most constraining grammar possible'.

We may now understand the setting of the various parameters relatively, viz., by interpreting the Subset Principle as follows: the unmarked values must be chosen so that they automatically allow the minimum of outputs. This seems to impose on us the following three-way assignment:

3.3.1.1. [+] defaults.
'Constraint' parameters like the Bounding Nodes for Subjacency must all be set [+] to guarantee minimal outputs: should Lx in fact allow more than these minimal outputs (as, e.g., S' but not S is a Bounding node in Italian), the positive evidence triggers the reversal to [-] for the node S.

Considering the 'destructive' nature of constraints, there might be a parallel to early phonology, where a cumulation of natural processes results in extreme poverty of outputs. Ceteris paribus, this would suggest that all

possibly-cumulative syntactic constraints are automatically active, with all parameters at [+] at acquisition-onset; the Unmarked values for Bounding nodes are thus all 'plus'.

Compare Subject-pro-drop in casual speech, perhaps truly a parallel, as I demonstrated in Drachman 1975. But it may be that only in end-state casual speech does syntax show cumulative 'destruction' of the kind exemplified in developmental phonology; for it is characteristic of beginner speakers that they keep morphemes intact, even at the expense of morphophonemic alternations, and acquire casual-speech rules only later.

Thus we might hold that the supposed parallel with early phonology is spurious, for example insofar as the developmental constraints in phonology and syntax are of quite different kinds. Thus the way to more plentiful phonological outputs lies either in reversing the ordering between feeding-pairs of processes, or in suppressing individual processes; but the parameters of Subjacency only subserve a law in setting its boundary conditions for particular languages. The resetting of a [+] to a [-] might well parallel the Stampean suppression of a destructive process; but on the other hand there seems to be no syntactic analog to re-ordering of processes, a basic characteristic of developmental phonology.

Quite apart from these considerations, there is the matter of "heard and attended-to" triggers; after all, there is hardly a phonological analog to the distinction between 'dominant data-type' vs 'exotic data-type' that we shall invoke immediately below for syntax. Notice in particular that we can for phonology establish whether the stored representation of a given segment is intact, even in the absence of a distinctive output for that segment; consider common cases of the type 'bat' vs. 'bad'

/baet/ --> [baet], but /baed/ --> [bae:t]

with 'displaced' contrast.

3.3.1.2. [-] defaults.

On the other hand, take the parameters representing optional properties, such as that involved in the licensing of Preposition Stranding in English or Object pro-drop as a syntactically active process in Italian, or the presence or absence of clitics in L1: these must initially be set at [-], so that only positive evidence will activate them.

Note that according to the 'minimal outputs' criterion, we are driven to assuming a default value of [-] for Subject pro-drop, since the [+] value would extend the set of potential outputs. The claim in Hyams 1983 (based on English, Italian and German data) that the default value here must be [+], must necessarily now be reinterpreted. I take now three alternative reinterpretations, each appealing to a different auxiliary hypothesis.

Rizzi's assumption (1986: fn 27, pg.526) concerns the abstract possibility that initial access is constrained by severe working memory limitations that involve the dropping of various grammatical morphemes (including pronouns) from the initial linguistic representations.

By contrast, Hueber 1986, surveying pro-drop data specifically for German, claims that the data are in fact irrelevant to the problem of the default

value for pro-drop; rather, he maintains, configurational relations have not yet come into play at the stage at which 'pro-drop' first arises. Hummer's auxiliary hypothesis is thus that early German shows merely pre-syntactic Topic-loss.

Can we reinterpret the data without reintroducing a pre-syntactic development stage (Cf Marantz 1981)? Since both English and German show Casual speech pro-drop (for English cf Drachman 1975), we might well assume that the data in question simply results from overgeneralisation from Casual Speech. However, as Hummer points out (personal communication), early pro-drop in German even occurs in sentences with Object fronting (i.e., in non-sentence-initial position), while Casual pro-drop does not: thus this third alternative also proves less than secure.

3.3.2. On 'Dominant Data-types'.

An interesting problem arises in connection with the 'open' parameters, viz., that the empirical data seem sometimes to contradict a prediction that follows from our standpoint on constraining the grammar. To illustrate this, compare Preposition Stranding (hereafter P.S.) with Object pro-drop.

Take P.S. first. Since it makes for further outputs, we are bound to say that the default value for P.S. is [-]. Similarly, many languages lack syntactically active Object pro-drop, so its appearance in Italian must be marked; its default value is thus again [-]. But while the prediction for Object pro-drop holds up (viz., children do not produce Object pro-drop without overt inputs) the prediction for P.S. seems to be empirically false, for young children do not (as would be expected) commonly produce questions with Pied Piping of Preposition-Phrases, as in

'In which cupboard did you put my teddy-bear, Mummy?'

But in fact it is unreasonable to expect necessarily to witness the data for the (nevertheless present) unmarked value for P.S. To distinguish the two cases, we introduce the notion 'dominant data-type'. By this we mean that certain data types (e.g., simple questions out of Preposition Phrases) occur in the child's heard and understood input so early and so often that the parameter-value is already set before the relevant outputs are attempted. By contrast, the data for Object pro-drop is so exotic ('this leads to conclude the following', or 'Good music reconciles with oneself') that one predicts a quite late switching of the parameter value, so that early child utterances of Italian children should show the (unmarked) non-application.

4. Doubts on some basic assumptions.

4.1. On negative evidence.

To revert to the possible interpretation of the setting for Subjacency as [-] rather than [+]. Suppose it were [-]. Then to answer the question, why (at the relevant point in development) Subjacency violations do not occur, we might well question the putative principle concerning 'positive evidence only'.

It may be that, at least for problems whose solution is not urgent for the

beginner language-learner, there comes a point at which the continued absence of a certain type of structure is indeed appraised and acted upon (Cf. Chomsky 1981:9, and 16 fn.9). We will thus talk of 'significantly-absent data-type', as a kind of converse to the 'dominant data-type' situation mentioned above. Subjacency might, a-fortiori, be a case in point; we need only suppose that the appraisal has occurred before the point at which the relevant complexity of utterances is otherwise available.

A further indirect form of data relevant to the child's setting of parameters might be the occurrence of 'rescue' strategies, e.g., the otherwise-unjustified insertion of Resumptive pronouns. Thus, consider the relevance for Subjacency of story-teller questions to children like:

'Who do you wonder why she had to praise the emperor's clothes?'
'Who do you believe the story that the giant nearly killed him?'

4.2. On overgeneralisation.

Maybe the child does not necessarily choose the most constraining grammar, as is suggested by the presence of overgeneralisations in each domain; e.g.,

a) in morphology, as with 'went-ed', 'see-d'.
b) in word-semantics, as with 'mommy' applied to any woman.
c) Object pro-drop, sponsored by Subject pro-drop in Greek.
d) Perhaps Casual-speech pro-drop helps to trigger the pro-drop parameter in English acquisition, while Casual-speech Topic-loss does the same for German pro-drop during primary acquisition, as mentioned above.
e) Similarly, SOV order in German acquisition may be partly sponsored by sentences with Modals, including Imperatives, as in :

'Du sollst Dein Wurst essen!' 'Eat your sausage!'

All these candidates for 'overgeneralisation' have perhaps rather varied status. Thus, on the one hand the whole issue is perhaps moot wrt word-semantics. On the other hand, while it is perhaps true that the quantitatively most prominent area of over-generalisation is that of morphology, still, if this proves the case, it calls for an explanation!

4.3. On the supposed independence of parameters.

Where principles, or the values of their parameters intersect there will, just as in the phonology of casual speech, be cumulative effects. And the chances of such interaction being strong is greater if all the variables are contained in one module: Cf. Borer's 'inflectional' model, containing Case-relations, inflectional relations and Theta-role assignment (1984:15).

Further, parameters associated with the same principle might well show hierarchical properties; I think of the relation between NP, PP, S, and S' as potential Bounding Nodes for Subjacency.

However, if the values of parameters associated with different principles correlate rather than apply independently, then one of the thus correlated parameters might prove to be redundant.

4.4. On the status of certain supposed 'laws'.
Doubts about the Absolute (i.e., law-like) status of particular principles have been expressed. For example, cf. 4.4.1-3 below.

4.4.1. C-command.

C-command is parametrised in Chomsky 1981:166, with 'strong' command for trace government, but 'weak' command being relevant to trace binding. Still, one cannot imagine that (e.g.) the functions of the two variants might be reversed in some language.

4.4.2. Projection Principle.

Here we mention Hale and the non-configurational version of the Extended Projection Principle, taken up in Pesetsky 1982 wrt Russian Subjectless sentences (cf. Drachman 1986). Cf here the notion, developed in Rizzi 1986 wrt so-called 'Object-pro', that an argument may be 'missing' if its Theta-role is 'saturated' in the lexicon.

4.4.3. Theta Criterion.

The Theta Criterion (one A-position may acquire (only) one role) is seriously questioned in Jackendoff 1972, a position upheld in his 1986. Cf. Chomsky's proposal (1981:139, fn.14) to disarm this position.

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Notes toward a Semantic Simulation of a Fragment of Child Language

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Scenario

A boy of three, out with his mother, sees a strange dog some thirty yards away. He likes dogs and wishes to approach and pet it. He is also afraid that it will bite him, and, to a lesser degree, that it will jump up and lap his face. At this point, his mother says to him, "That dog is old." Since the sentence is a simple one, it can easily be parsed, and there are many parsing programs that will handle it quickly. The problem we wish to address is a semantic one. Once the child has resolved the sentence into its components, how will he interpret them? That is, how will he process them, and what difference will that processing make to his beliefs, intentions, and behavior?

While these questions are extremely difficult, we will suggest some ways in which a computer simulation of this aspect of the boy's functioning might be approached. We will then engage in some speculations about the reality to be simulated. Before proceeding to the semantics, there are some important phonetic assumptions that must be made. The mother's utterance will make no difference unless it is uttered within a certain range of tones of voice. Moreover, there may be some tones that would effectively forbid the boy to approach the dog, or which give him permission to do so, regardless of the words that are uttered. In these cases there will be no semantic processing. We hope to interest Ilse Lehiste, who is far more competent in this area than ourselves, in answering questions of this sort.

Let us here assume that the sentence is uttered in such a way that the child listens to it and takes it seriously, but still feels free to decide how to deal with the dog. It must now be recognized that the boy already has a great many beliefs about the world in general, and about dogs in particular. The instant he sees the dog, he will begin to apply as many of these beliefs as possible to the present case. Our simulation will therefore assume an existing database and a method of generating predictions about the dog. The importance of "That dog is old", as received and parsed, is that it will alter these previously existing beliefs in ways to be discussed. If one felt compelled to ask what the sentence means (in a philosophical way), or what its semantic content is, one would be asking for a generalization about the ways in which it affects the existing beliefs of individuals. Such questions are not particularly useful.

A simulation of the child must contain a parser which is capable of isolating the subject, no great problem in the case of such a simple sentence. Once "that dog" is returned from the subject search, the general problem would be to find out what it refers to on this occasion. We here hypothesize that the child's problem is much simpler than this might seem. He cares only about the question he already has in mind, whether to approach the dog. He is not interested, at such a moment, in storing general information which may, or may not, be useful later on. He thus assumes that "that dog" refers to the object of his current interest, the dog, and will make only a minimal check. In order

to do this he must have a database in which "dog" is associated with some of the observable features of a dog. If a certain proportion of these features are not observed, the whole sentence is thrown out as being of no current interest.

The most important of the boy's beliefs about the dog probably do not concern such things as its color and size. They are expectations concerning the behavior of the dog when approached in various ways. One way of putting it is that there is a procedure which the child expects the dog to follow. It would seem that very young children can have rather elaborate expectations about the behavior of persons and animals. Most important from our point of view, these expectations can be modified by verbal input.

There are, at this point, two ways of looking at the situation. One can think of the child as expecting the dog to follow a program with many sub-routines, each of which concerns the behavior of the dog in some hypothetical situation. On the other hand, one can think of each sub-routine merely as representing a dispositional property on the part of the dog. For example, "bad-tempered" means, more or less, that the dog will bite in a certain range of circumstances, growl in others, and so on. In one sense, it makes little difference whether we speak of a dispositional property or a program. On another level, however, it makes a great deal of difference. If we stick to properties, the program that the child follows can simply chain them together, allowing that the links in the chain are only statistical, and much less than foolproof. When verbal input, such as "That dog is old" comes along, it can be allowed to affect the chains, that is, the data.

Alternatively, if we have sub-routines instead of dispositional properties, we are likely to have fewer of them. One sub-routine is altered in certain ways to make it represent a new and different dispositional property. For example, an extremely bad-tempered dog follows the same basic program as a bad-tempered one, except that it takes less provocation to make him growl and bite. It might seem, then, that it is more economical to choose a few sub-routines which, with seemingly minor modifications, will represent a large number of dispositional properties. If, on the other hand, each dispositional property is taken as independent, the master program that the child follows will not "know" about the connections between those properties (and the programs corresponding to them). There is, however, one great difficulty in the program approach. It is extremely difficult to set up a general program to modify sub-programs. It may be virtually impossible to get the degree of generality to handle economically the changes the child would have to bring about in the sub-routines when he gets verbal input, as in our example. It is much easier to effect alterations in chains. It will be more economical, in the long run, to ignore or "lose" a certain body of information (the relative degree of similarity or overlap between dispositional properties), but, at the same time, avoid the pitfalls of writing programs to alter other programs.

Let us take the following set of items as an example of a fragment of our database.

[OLD][~YOUNG]
[YOUNG][ACTIVE]
[ACTIVE][MAKE NOISE]
/DOG\[ACTIVE][JUMP UP]

/DOG\[ACTIVE][BITE]
/DOG\[BARK][MAKE NOISE]
/DOG\[JUMP UP][LICK]
[FRIENDLY][~BITE]

The input from the mother (root item) will be in subject-predicate form, and the subject, here DOG, may well refer to a particular dog. However, the words appearing in database items refer only to general properties, and the item itself is merely the record of one or more observed co-occurrence of those properties. The order of the words in a database item (but not a root item) will thus make no difference. We also assume that the child makes no distinction between the general and particular uses of DOG. Nothing in the procedures to come will depend on it, and we are suggesting that the most rudimentary and fastest-acting system best fits the needs of the child at a certain stage.

One could certainly hypothesize that there is another (perhaps later) database containing information in subject-predicate form, but we will look first to the minimal model. Even this database does contain a feature which does some of the work of predication. Anything enclosed in /\'s is a non-exchangeable matching word which must appear in the string under consideration if this particular item is to be used. The chaining algorithm uses these items to generate transformations of the original input. It works along the following lines (entries from the database are enclosed in {}'s):

```
[DOG][OLD] (root)
    {[OLD][~YOUNG]}
[DOG]     [~YOUNG]
          {[YOUNG][ACTIVE]}
[DOG]               [~ACTIVE]
                    {/DOG\[ACTIVE][JUMP UP]}
[DOG]                          [~JUMP UP]
                               {/DOG\[JUMP UP][LICK]}
[DOG]                                     [~LICK]
```

We have, in effect, allowed the inference from {[YOUNG][ACTIVE]} to {[~YOUNG][~ACTIVE]}. While this sort of inference can cause problems, we have here in mind a context so limited that allowing it will do more good than harm in terms of efficiency. Since there are many transformations which can be made, we have to specify an objective. Let "%" be defined to be the logical equivalent of "plus or minus". Then define the objective as being of the form [%A][%B]... or [A][%B][%C]... For example, [DOG][JUMP UP] or [DOG][~JUMP UP], the two answers that the child is interested in, are of the form [DOG][%JUMP UP]. We will later suggest an algorithm capable of selecting an appropriate path to the end result.

The child is likely to receive information that conflicts with his previous beliefs. His mother's input will create the root [DOG][OLD], but he may have {[DOG][YOUNG]} or {[DOG][~OLD]} in his database, thus believing, in effect, that all dogs in his environment are young. He would therefore have to choose between the new information and the old. If we build our model in that way, the child being represented must be either excessively susceptible to suggestion or immune to it altogether. In fact, when the mother says that the dog is old, that should induce a slight increase in the child's accep-

tance of the dog. It should not produce a response as if the child had been on intimate terms with the dog since birth. What we want is an increment which, when repeated, produces a belief of increasing strength. The simplest way to achieve this is to give the proposition, not a truth value, but something like a probability, which, being continuous, can have an infinitely fine variation of values. Let us therefore introduce a statistical measure of association, "&", which has a range of -1 to 1 inclusive. The co-efficient, -1, when attached to a word, represents the situation where the property is believed (with practical but not absolute certainty) not to be present, and 1 that where the property is similarly believed to be present. The value 0 implies no belief either way. If we use this "&" in place of the "%" , it will have certain useful properties. Double negations will cancel, and, when we multiply co-efficients, a chain of reasoning built on a series of dubious assumptions will reflect the cumulative uncertainty of the whole. The calculated value of "&" will have a sign which is, in a sense, a result. It will also have a magnitude, which is the reliability of that result. Our new data base look like this:

```
[(1)OLD][(-1)YOUNG]
[(1)YOUNG][(.9)ACTIVE]
[(1)ACTIVE][(.9)MAKE NOISE]
[(1)ACTIVE][(.9)JUMP UP]
[(1)ACTIVE][(.1)BITE]
/(.2)DOG\[(.9)BARK][(.9)MAKE NOISE]
/(.2)DOG\[(.9)JUMP UP][(.8)LICK]
[(1)FRIENDLY][(-.95)BITE]
```

The non-substitutable matching word (in /\'s) now has an associated factor which must be used in computing "&" if the item is used under conditions where the matching word does not appear. e.g using {/(x)A\[(y)B][(z)C]}, [(j)A][(m)D][(n)B] yields [(j)A][(m)D][(n*y*z)C], but [(m)D][(n)B] becomes [(m)D][(n*x*y*z)C].

Note: For purposes of computation we can add to an item any substituteable word with a co-efficient of 0 or any non-substituteable word with a co-efficient of 1.

We now have a derivation like this:

```
[(1)DOG][(.9)OLD]                          (root)
        {[(1)OLD][(-1)YOUNG]}
[(1)DOG]          [(-.9)YOUNG]
                   {[(1)YOUNG][(.9)ACTIVE]}
[(1)DOG]                     [(-.81)ACTIVE]
                               {[(1)ACTIVE][(.9)JUMP UP]}
[(1)DOG]                                   [(-.729)JUMP UP]
```

When we use the database, coefficients are always multiplied together, and, within that application, have no separate importance. However, when the mother (or anyone) speaks to the child, the coefficients in the root item have a different significance. In [(1)DOG][(.9)OLD] we assign 1 to DOG since the child assumes its presence and has his attention centered on it. The other coefficient is a measure of confidence the child has in this particular speaker before he consults his own database. The result of the derivation,

[(1)DOG][(-.729)JUMP UP], does not, in itself, imply an approach to the dog, but would be one component in a larger model that might represent desires as well as beliefs. Having reconciled them, it would produce output which represents intentional actions. It is worth noting, however, that the model which produces the best output may not be one which preserves the ordinary distinction between desire and belief.

Let us now turn to the database itself and ask how it might be formed. There must, in the beginning, be categories. A child is more likely to recognise and remember a cat than an object which comprises, say, the lower 60% of the cat and three square feet of the surface on which it is standing. Philosophically, there is nothing wrong with the latter sort of object, but it is unsuited for our model because, if it were a category, it would give rise to a less useful database than the sort the child seems to have.

There will be a word associated with each category, and the general principle is that, whenever the child has a sufficiently striking experience, a new item is created. If he notices only an active squirrel, SQUIRREL and ACTIVE will both have positive coefficients. If he notices a young man with a hat, and notices that he has no coat, YOUNG, MAN, and HAT will have positive co effiencts and COAT a negative one. The magnitude of the coefficients will depend on the extent to which the child is "struck" by each feature, or by combinations of them. This allows for the representation of non-rational factors. The child may, for example, be intensely affected by an object or aspect of an object because he fears it, and this may predispose him to expect its re-occurrence. Another possibility is that the child may not be impressed by an experienced combination at a given conscious or unconscious level, yet repetition may still heve its effect. Thus, on the tenth occurrence of the combination, he may "feel" that the two factors which are then co-present will always co-occur. In that case both coefficients will be higher than they would otherwise be. A completely developed model would have to have some mechanism for measuring these factors and deciding what sort of environment and prior condition of the child would give rise to an input which is striking to one degree or another. It may ultimately be found that it is better to simulate a whole environment with a number of persons in it, as opposed to constructing a model for the child alone. For the present, we would envision a series of models representing a single individual, beginning with extremely simplistic ones, but which would gradually grow more sophisticated. The algorithms used to set coefficients would mirror that development.

This process of database development will, in the course of time, produce items which have the same words but different coefficients. In reconciling those differences we must remember that it is not a matter of averaging them. If we have both {[(1) DOG][(.75) OLD]} and {[(1) DOG][(.65) OLD]}, we must remember that the second item provides additional confirmation for the first, and vice versa, so that the reconciled coefficient for OLD ought to be higher than in either previous instance. We will therefore need an algorithm for so handling items in the data base, and for reconciling them with new information, as, for example, that which comes from the mother.

We can think of this process as one of "churning the database", and it is stimulated, not only by new input, but by many other occurrences. Since each new item must be "bounced off" and reconciled with each relevant old item, there is a natural conservatism which favors a considerable body of old infor-

mation (subject to qualifications to be made later) over new information. Churning is suspended each time there is a need for action, and thus for definite coefficients. When that happens, the relevant database item most easily reached is used, thus importing a random element into the resulting beliefs and actions. As a churning algorithm, we suggest the following:

Let a && b = a+b+c(a,b)
where c(a,b) = -a*abs(b) if a*b > 0, else c(a,b) = 0

Then, taking the item from the database to be [/(x)A\[(y)B][(z)C..]], and linking from B to C,
[(j)A][(m)D][(n)B][(p)C..] becomes [(j)A][(m)D][(n)B][(r)C..]
where r = (p && ((x && abs(j))*n*y*z)),
but, if A does not appear in the derived root item (j is 0),
[(m)D][(n)B][(p)C..] becomes [(m)D][(n)B][(r)C..]
where now, r = (p && (n*x*y*z))
In either case, the coefficient z in the database is replaced by w : w = z+e*((y*r/n)-z) where e=abs(p&&(-r)).

The fact that some of the algorithms required for these tasks in the model may be complex does not imply a claim that the child does complex calculations in his head. These and other algorithms are arrived at by setting forth plausible cases, plotting them, and then finding a formula that fits the curve. The result might be taken to describe a neural electro-chemical process within the brain. In all models of this sort there are many algorithms used in the computation which can be progressively modified and adjusted to produce results more nearly corresponding to the observed reality being modelled. The battle is largely won if the model is flexible in enough ways so that the results can be skewed in the desired direction by changes of algorithm.

A critical question in this sort of model construction is: How long should items be held in the database? We have argued elsewhere (Todd, Thompson and Todd 1984: Part 6) that human reasoning is more likely to suffer from too much information than from too little. The child needs a system that works fast. It is better to supply the need for action with conclusions, even if a significant percentage are false, than to have action delayed or stultified by too much processing. We also suggested there that certain phenomena of aphasia can be understood best on the assumption of a periodic wipe-out of most of the database while, at the same time, new conclusions are constantly being generated. It is often better to generate a conclusion anew than to store it indefinitely, particularly since the coefficients need periodic revision in any case. This kind of periodic wipe-out will lose connections which would have been "confirmed" if the timing of the wipe-out cycle had been different. But, again, minimal information loss is to be tolerated in the interests of speed and simplicity. At least, that is the hypothesis about the child embodied in our model. We will again leave open the exact procedure for deleting items from the data base on this ground.

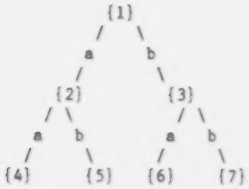
In scientific investigation, some concepts, such as that of density, have turned out to be inordinately productive. At the opposite extreme are concepts such as Nelson Goodman's "emeruby", denoting an object that abruptly changes from an emerald to a ruby at time t (Goodman 1965: 102-3). If t is taken as the present, any evidence which confirms the belief that an object is an emerald equally confirms the belief that it is an emeruby (and hence will change color, etc. immediately). An emeruby is, of course, an extreme case.

There are indefinitely many other concepts which are, to one degree or another, unsuited for scientific or everyday reasoning. Goodman has shown that there is no logical or inductively justified way of ruling such properties out of court. We may not like them or use them, but science itself gives us no reason for rejecting them. A consequence of Goodman's point is that the child, "looking over his concepts", has no way of knowing which may be, to some degree, like that of an emeruby. His only real guide will be the input he gets from others. Thus, a tally must be kept of the frequency with which each word denoting a category in his database is spoken to him by others. Thus, in addition to the UP-Dating Algorithm and Churning Algorithm, there will have to be a Lack-of-Frequency Algorithm which systematically lowers the coefficients of such words wherever they appear in the database. If we now, at the periodic wipe-out phase, eliminatate, roughly speaking, all items the products of whose coefficients are distanced from 0 by less than a given threshold, the database will be skewed in favor of the concepts used by the larger society.

We have seen that working with the database changes the database. We must therefore have a means of restoring the database to the state that it would have been in if we had not done the last x transformations. The simplest way to do this is to treat a change or changes as a series of wholesale insertions and deletions of items, the series being stored in a stack which exists for that purpose. These are all reversible so, to go back up the tree structure of possible transformations towards the starting point, we merely take entries from the stack, insert the deletions, and delete the insertions.

Suppose now that we want to use two or more external roots. We will have a separate external root database in which these are put and it will be temporarily appended to the main database. We will then start transforming one item with the use of the others. If all the items in the external root database are used then the derived result can be said to have been derived from them. It is, of course, possible that one of the items in the external root database will be totally unrelated to the subject at hand, and, in that case, it cannot be incorporated in a chain leading to the desired result.

Let us consider each possible state of the database and derived root item as a node in a branching structure with the branches being different possible transformations of the database and derived root item in the state associated with the node from which the branch issues. The branching structure would look rather like this:



where the nodes {1}, {2}, {3}, {4}, {5}, {6}, and {7} are possible states of the database and derived root item, and the branches a and b are possible transformations of the same.

Let us next consider searching all possible combinations of items or,

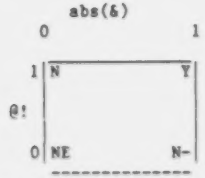
rather, some reasonable subset of them. This is where "&" comes into its own. Consider a quantity called "&*" and let "&*" be the product of all the "&"s of the derived root item. At this point, &* obviously pertains to the whole derived root item, rather than to a part of it. If "&*" falls below a certain threshold, then we branch back and try a different branch from the previous node. If all the branches from that node are untenable, then we branch back still farther, and so on. To ensure that the first items, comprising the external root database, are used, we have the rule that possible branches are considered in the order that they appear in the database.

We now have a scheme which searches for what, speaking somewhat loosely, amounts to the set of statistically significant implications of the original state of the database, together with the external items, with special stress being placed on the implications of the latter. But this is not yet what we want. We want to know, not only whether the derivation is reasonable, but whether it is relevent. As stated before, we have a target item of a form similar to the items in the database except that it does not have "&" coefficiants. It may however have weights, which we shall call "@!", taking 0 to mean that the word does not appear in the target and 1 to mean that it is fully present. The object is to determine which of its words should get preference in being matched with words in the derived root item. Further, we have some statistic which we shall call "&#" for measuring closeness of fit between the target and the derived root item. One possible formula would be the following:

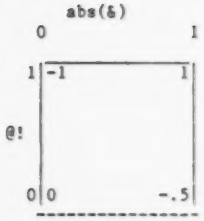
&# = sum of &#(j) for all possible words (where &#(j) is a measure of fit between the occurrence of a word in the derived root item and the occurance of that same word in the target.)

```
&#(j) is computed as follows:
  if @! >= abs(&) then
   &#(j) = 2 * abs(&) - @!
  otherwise
    &#(j) = 1.5 * @! - .5 * abs(&)
```

This formula was obtained by taking four cases of abs(&) and @!, intuitively selecting appropriate values of &#(j) for them and then contriving a function to fit them. Here are the four cases plotted on a graph. It should be noted that the linking together of word computations is effected by addition. Therefore the identity element is 0. Wth symbolic values, the graph is:



where N is no, Y is yes, NE is no effect, and N- is no, only less emphatic than N. With numbers, bearing in mind that NE must be 0,



The result is a system that finds what are, in effect, statistical inferences about the relevance of the root item, based on new information. It should be noted that these are not definitive, as the number of items which can be derived is not finite, and therefore we search only a small subset of the possible range of combinations.

We would like to treat briefly the means whereby the algorithm described above would be implemented in hardware in what might be called a realistic case, by which we mean a case involving much larger amounts of data. This may serve to give some insight into the sort of processes going on inside a child's brain. Let us consider that the child is at a node called {A} in the algorithm above and let us consider that {A} has daughter nodes {B}, reached by branch b, {C}, reached by branch c, {D}, reached by branch d, and so on. It should be understood that the limits of speed in going through the algorithm are not posed by the total amount of computation to be done, but by the number of things which must be done in sequence. If many different parts of the job do not depend on each other for inputs, they may be done at the same time by different equipment. That said, let us assume that there are processors available for each of the branches b, c, and d. First, each of them must receive a copy of the information making up node {A}, that is a complete copy of the database, a complete copy of the change stack, and the derived root item. While this may seem an impossible amount of material to transfer, it can all be sent at the same time if the data path is broad enough. There is no reason why this should not be the case, as it only means that the data path or what would be called the bus in a computer must be on about the same scale, the same order of complexity, as the storage medium.

Let the processors execute the branches on their copies of the node {A} and generate "&#" for the daughter nodes. The results determine whether the search will continue through that node or not. If that node is not a good candidate for continued development, its processor will then be released to a common pool of unemployed processors. If, on the other hand it is worthy of development, the paths leading to its own daughter nodes will be allocated processors from the pool, if they are available. If not enough are available, the available processor or processors will work through the paths in sequence as required. This approach is standard practice and is different only in scale and not in kind from the facilities available on most large mainframe and supermini computers. It will be noted that we use an underlying mechanism which is very simple of itself, in that there is no attempt to predict which branches are worthy of development.

In conclusion, it should be remarked that the suggested model would

occupy a position in two different series of models. While it intentionally ignores many distinctions to be found in natural language, the result is a high degree of simplicity and speed of operation. There are, of course, many kinds of simplicity, some of which conflict with others, but we have chosen the kinds we think appropriate at this stage of language acquisition. One series of models then represents different stages of acquisition, terminating with full adult competence. Our larger speculation is that, starting with a model such as that outlined here, subsequent ones can be fitted with additional features without there ever being a need for a radical re-design.

The other series of models, starting from our outline, represents improved attempts to simulate a given level of linguistic competence. We have suggested that a great deal can be done by improving the algorithms. However, the important thing is to work toward an actual computer model whose input and output can be compared with that of the child. Then, even if the results do not tally, we would be in a position to work toward a radically improved model.

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Some Literary Manifestations of Language Contact

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Linguistic approaches to literature have been pursued for a number of years. Studies focusing upon fiction, poetry, or specific literary strategies such as metaphor, have utilized diverse linguistic theories; e.g. structuralism (Culler 1975), dialectology (Page 1973), transformational grammar (Ohmann 1964), and pragmatics (Pratt 1977), to name a few.

However, there exists a dearth of studies which take up the application of sociolinguistic concepts to literary analysis. Noteworthy here are Sarkany (1974) and research in Production littéraire et situations de contacts interethniques (1974). Yet much remains to be done. The present study explores the literary employment of certain linguistic behaviors and attitudes which, in real speech communities, presuppose language contact. I will look at the French Canadian author Gérard Bessette's novel, Les Pédagogues (1961), in the light of perspectives associated with language contact.

Language contact implies individual bilingualism: two or more languages are said to be in contact "if they are used alternately by the same persons. The language-using individuals are thus the locus of the contact" (Weinreich 1968:1). But although the two codes in a bilingual's repertoire alternate, they are rarely equal in social function or value within the speech community wherein the bilingual interacts with others. Typically, in some domains one language is preferred over the other. Moreover, it is frequently the case, especially in urban industrialized societies, that the language of intimate domains such as family and friendship is not the language of the society at large. In such a case, the home language is regarded, at the macrosocietal level, as a minority language. Accordingly, the minority language and the majority language come to be associated with distinct values, purposes, and import. As Gumperz indicates:

> The tendency is for the ethnically specific, minority language to be regarded as the 'we code' and become associated with in-group and informal activities, and for the majority language to serve as the 'they code' associated with the more formal, stiffer and less personal out-group relations. (Gumperz 1982:66)

These differing connotations of the two codes may be realized, at the level of individual behavior, either as a set of overt or covert attitudes regarding the codes or their speakers; or as conversational strategies wherein the bilingual speaker might--within a particular speech exchange--wish not only to convey a specific message, but also simultaneously to imply a value associated with the particular code chosen during that exchange.

It should be added that a writer, as a member of a given speech community, understands the linguistic and nonlinguistic conventions of his community. A literary text, as social product, is grounded in those conventions. Moreover, readers who are members of the same community also understand these conventions. Without this common knowledge between writers

and readers, literary communication would be impossible. Fowler underscores this point, when he indicates that an author "can write meaningfully only within the possibilities provided by the systems of conventions which define the culture" (Fowler 1977:125; emphasis added).

In a speech community characterized by language contact, one set of conventions involves appropriate use of the respective languages, along with attitudes regarding their differential social value. I turn now to Les Pédagogues, a novel in which the author utilized language contact as a literary strategy for defining character and presenting theme. (Because of space limitations, this analysis cannot be exhaustive.[1])

Bessette's Les Pédagogues is a novel written in the tradition of French literary realism. It tells the story of five professors of the fictional École Pédagogique de Montréal who are oppressed by what they perceive as the mediocrity and clericalism of the Québec educational system. The solution which the author proposes--through the protagonist--is for the teachers to unionize, more specifically to join the blue-collar workers under a larger union encompassing both intellectuals and manual laborers. The novel's protagonist, Sarto Pellerin, head of the École's French Department, is thrust into the leading position as unionizer of the teachers.

Although the setting and language of this novel are predominantly French (the action unfolds in Montréal), the French and English languages take on significant literary value in light of the differing social functions of the two codes in the real speech community underlying the fictional universe of Les Pédagogues. However, although the novel is written in the tradition of literary realism, language use in the novel does not precisely mirror all actual use as it exists in the real community.

In Canada, where English dominates as the language of economic and political life, French is a minority language. Even in the province of Québec, English and its speakers occupy a pre-eminent position and enjoy great prestige. As Basham writes:

> French Canadians feel and know that in order to advance within Canadian society, including French Canadian society, a mastery of English is virtually indispensable. Everywhere in Québec, except perhaps in such completely rural areas as the lowest part of the St. Lawrence Valley, English exerts a pressure far, far in excess of the numerical importance of the English speakers of the region. (Basham 1978:85)

As a consequence of the subordinate status of French in Canada, many French Canadians have developed negative attitudes towards their language and group. (Research on the sociolinguistics of this issue is reviewed in Giles and Powesland 1975; see also Basham 1978.)

A joyless outlook on French Canadian culture is the dominant motif of Les Pédagogues, and it is the pédagogues themselves who censure the culture. One object of attack is an irrational clericalism, which stresses conformity

at the expense of intellectual creativity, social harmony at the cost of social cultivation, and marital fidelity at the cost of conjugal satisfaction. Other sources of dissatisfaction are more immediate to the professors' daily lives: the men are poorly paid, so moonlighting is common. Moreover, as their influence in decision-making at the École is minimal, they lack the opportunity to improve their lot. The disaffection engendered by these problems is manifested in various ways among the men.

Yves Lambert, professor of music, is also a pianist who plays publicly. But because he cannot find sufficient nourishment in the culture for his artistic aspirations, he flounders as a performer. He confesses to feeling in Québec like un poisson dans l'air 'a fish out of water' (LP, p. 65). In order to compensate for his professional mediocrity, Lambert, a bachelor, seeks victories in the area of 'amour.' He has a mistress, Annabelle, a former ballerina from France, who is convinced that Lambert enjoys a breadth of social experience which she can use to launch herself into the highest social circles. In order to encourage this illusion, Lambert occasionally code switches, from French to English, in intimate conversations with her. He will, for example, call her darling instead of ma chère. Lambert is pretentious, and the prestige associated with English is supposed to impress Annabelle.

For the professor of English, John Sloper, English represents economic survival in the most basic sense. Although Sloper draws a salary, because of medical expenses incurred by his sick wife, Sloper moonlights as a private tutor of English. His clients are French Canadians, and the more well-to-do they are, the better it suits Sloper. Sloper is ashamed of his economic inadequacy, so he rationalizes on every possible occasion. For instance, he cannot afford a car, but he explains his walking--when others might drive or take a taxicab--as a health habit: walking, he affirms, is sain, naturel 'healthful, natural' (LP, p. 237). Because opportunities to teach English privately are all that stand between Sloper and penury, one is not convinced that intellectual integrity underlies this professor's declaration that every Canadian ought to be bilingual. Sloper's stand here is so radical, that he would also like to see public assistance rendered to the poor and unemployed only on condition that they learn the two languages. As he states before a group of acquaintances:

> Nous vivons dans un pays bilingue. Par conséquent, tout le monde devrait pouvoir s'exprimer dans les deux langues. Ce serait la première condition. Ceux qui refuseraient d'apprendre soit l'anglais, soit le français, selon le cas, ne devraient pas recevoir d'aide. (LP, p. 271)
>
> 'We live in a bilingual country. Consequently, everyone ought to be able to express himself in the two languages. This would be the first requirement. Those who refused to learn either English or French, whatever the case, shouldn't receive assistance.'

Thus for Sloper and Lambert, English connotes economic survival and prestige, respectively.

Canadian French, on the other hand, elicits reactions different from these. Although as a code it is not negatively assessed in relation to English, careless or vulgar French evokes the ire of linguistically sensitive men. The loudest voice arguing for good French is that of Sarto Pellerin, the protagonist, who is head of the École's French Department. Pellerin's own French is extremely correct, even in intimate settings wherein, in real life, colloquial structures would be appropriate. For instance, Pellerin's language is dominated by multiple negation (ne...pas, ne...rien, etc.), which is formal. A typical sentence for Pellerin is Il n'a pas dû être content... 'He could not have been happy ...' (LF, p. 82). In this, his speech contrasts with that of his wife, a simple country woman, whose language is more colloquial. For her, a typical sentence is Tu dois rien à Paul, tu sais 'You don't owe Paul anything, you know' (LF, p. 82), in which ne...rien is reduced to rien. (On multiple and reduced negations in real Montréal speech, see Sankoff and Vincent 1980.) This is one of several linguistic distinctions that underscore the disparity in social class between Pellerin and his wife. The couple are ill-matched, and both are unhappy with each other.

Pellerin's French, together with his passion for good French, is the linguistic manifestation of his desire to see his culture elevated, to see it freed from mediocrity of every sort. Although the character of Pellerin is somewhat overdrawn, this pédagogue nevertheless speaks for the author. Bessette is suggesting that the answer to Québec's cultural ills lies not in Lambert's pretentiousness nor in Sloper's conservatism--both expressions of escapism--but in a sincere commitment to social ennoblement. This is why Professor Pellerin speaks the best French--and ultimately joins the trade union movement.

Notes

1. Some of the data here are expanded and clarified in my research project, in progress: The Application of Sociolinguistics to Literary Analysis.

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'Concessive' as a discourse relation in expository written English

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1. Introduction

Concessive constructions have almost exclusively been studied in semantic terms, often by referring to the notion of 'surprise'.[1]

My approach in this paper will be different: I wish to examine concession as an interactive discourse relation, defining it in terms of writers' goals and readers' perceptions of these goals, and to explore the implications of viewing concession this way rather than strictly in semantic terms.

2. Previous research on concession

One of the earliest and most influential definitions of concession is that offered by Quirk (1954:6): '... the concessive relation may be said to exist between two parts of an utterance when one part is surprising in view of the other.'

A variation of this definition can be seen in Quirk et al (1972:874): 'Concessive conjuncts signal the unexpected, surprising nature of what is being said in view of what was said before', and in Quirk et al (1985:1098): 'Concessive clauses indicate that the situation in the matrix clause is contrary to expectation in the light of what is said in the concessive clause.'

Winter (1982:107-117), looking at actual texts, accepts Quirk et al.'s 1972 definition, and proposes that the differences between although and but as signals of concession are best discussed in terms of 'known' and 'new' information.

König (1985) and (to appear) suggests a further semantic property of concessive sentences: 'there is an incompatibility or conflict between the facts described by p and q' (1985:4), which is also mentioned in Harris (to appear): 'the antecedent marks an extreme value (whether potential or actual, depending on the clause type) within a set of possibilities, a value generally taken to be incompatible with the consequent.'

Shared by all the grammarians cited so far is the statement of the 'surprise' or 'incompatibility' in absolute terms; that is, it is not considered who is supposed to be surprised or to perceive the incompatibility. Further, none of these definitions distinguishes what we consider to be neutral contrast from concession, since contrast may also involve 'surprise' or 'incompatibility'. In fact, König (to appear), notes that 'many investigations that have struggled with this problem [of distinguishing 'concessive relations' from 'adversative relations'] have come to the conclusion that a clear distinction between these two types of relations or types of connectives cannot be drawn and I will therefore speak

indiscriminately of "concessive" or "adversative" relations.'

Jordan (1985) represents a broadening of the discussion of concessives in his explicit mention of the writer as responsible for signalling the 'surprise'. Jordan compares concession with other types of 'counter-expectation' in actual texts in terms of 'signaling transition between types of information' (p. 265). Accepting as a working definition Quirk's (1954) definition, he discusses the implications of several examples in which although 'the rebuttals are surprising in concessive terms ..., they also very clearly contain predicted information, which is thus hardly surprising in view of what was said before' (p. 11). Jordan goes on to discuss relations of surprise and expectation in terms of transitions from one 'type of information' to another.

In this paper, I wish to suggest that another perspective on the concessive relation may be gained by avoiding the semantic notion of 'surprise' altogether and focussing instead on what we can assume writers are doing with texts.[2]

3. Relations in Discourse Structure

It is uncontroversial that discourse is coherent, and that parts of a discourse 'go together' to form a whole. As background to an analysis of concession, I will consider one type of discourse, small written expository texts in English, and describe one factor involved in the creating and interpreting of such texts as coherent. This factor is the existence of perceived organizational, or rhetorical, relations between parts of the text.

These relations, often not directly signalled, are essential to the functioning of the text as a means for a writer to accomplish certain goals. These relations involve every non-embedded clause in the text and they form a pattern of relations which connects all the clauses together.

Let's begin by considering an example for illustration. The following short text has been broken down into 'units'; each unit consists of one clause, except that embedded complement and relative clauses are considered part of the same unit as the main clauses with which they are associated.

(from a researcher at ISI, an artificial intelligence research organization; message appeared on the ISI electronic bulletin board:)

1. I am having my car repaired in Santa Monica this Thursday 19th.

2. Would anyone be able to bring me to ISI from there in the morning

3. or drop me back there by 5pm please?

In this short text, Unit 1 poses a problem, to which a solution, the request expressed in Units 2 - 3, is proposed. We can thus postulate a SOLUTIONHOOD relation between Unit 1 and Units 2 - 3. Such judgements are inferences made

on the basis of various types of knowledge which readers bring to texts; as readers, we infer what the writer's purposes must have been. Our definitions below explicitly acknowledge that our analyses involve judgements of plausible writer goals.

Other relations which have been discussed in the literature referred to above include CONDITION, BACKGROUND, MOTIVATION, CIRCUMSTANCE, ANTITHESIS, CONTRAST, ELABORATION, and, the focus of this paper, CONCESSION.[3]

I would like to suggest that the much-discussed clause-combining domain of 'frustrated expectation' or 'counterexpectation' be divided into three sub-domains, according to discourse function: ANTITHESIS (see Thompson and Mann (to appear)), CONTRAST, and CONCESSION.

4. CONCESSION as a discourse relation

The definition of CONCESSION which I would like to propose incorporates the element of 'incompatibility' of König and Harris, mentioned above, but differs from them in viewing the incompatibility as potential or apparent and in relating it to the writer's purposes rather than taking it as some kind of absolute. But before I can present this definition, I must introduce the concept of 'positive regard'. Writers pursue different sorts of goals with different texts and text spans. Some are intended to persuade, i.e., to create belief. Others are intended to create an attitude of approval or interest. Still others are intended to create desire, an intention to act. These are all varieties of what we might call positive regard. In analyzing any one text span and decomposing it into parts, we use a single primary notion of positive regard, either belief, approval, or desire, with the particular choice of notion depending on the analyst's perception of the writer's intent.

The CONCESSION relation can be said to hold between two parts of a text, a and b (where b is the part doing the conceding), if it is plausible that the writer:

1. has positive regard for a and wants the reader to have positive regard for a too;
2. acknowledges a potential or apparent incompatibility between the situations presented in a and b;
3. regards the situations presented in a and b as compatible;
4. believes that the reader's recognizing this compatibility will increase the reader's positive regard for a, in that the reader will be less likely to discount a in the face of possible objections to it.

Before considering some of the implications of this textual perspective

on the concessive relation, let's look at three examples. The first is taken from a 19-unit description of one of the announcers on a Los Angeles public radio station:

17. Although Jim lists tennis, Chinese food, and travel to exotic locales among his favorite hobbies,

18. one can't help but wonder at the unmentioned interests that help spark Jim's creativity, leading him to concoct an unending stream of imaginative programs.

In this extract, all the conditions are met for taking unit 17 to be in a concessive relation with unit 18. It is plausible that the writer:

1. has positive regard for the likelihood that Jim has unmentioned interests sparking his creativity (unit 18), and wants the reader to do so too;

2. acknowledges the apparent incompatibility between listing only three hobbies and the likelihood of having a wider range of unmentioned interests;

3. views listing only three hobbies and the likelihood of having a wider range of unmentioned interests as in fact being compatible;

4. believes that the reader's recognizing this compatibility will increase the reader's inclination to have positive regard for unit 18 too, since the reader is less likely to object, 'but only three hobbies are listed.'.

In this extract, the concessive relation is signalled by means of the hypotactic concessive conjunction although. But there are other ways of signalling this relation. The following example involves a paratactic construction with but; this extract is from the beginning of a personal letter:

1. Your kind invitation to come and enjoy cooler climes is so tempting,

2. but I have been waiting to learn the outcome of medical diagnosis

3. and the next 3 months will be spent having the main thumb joints replaced with plastic ones.

Here unit 1 is in a concessive relation with units 2-3. Once again, it is plausible to analyze this text in terms of a writer who:

1. has positive regard for units 2-3, the necessity of thumb surgery, and wants the reader to do so too;

2. acknowledges the potential incompatibility between the temptation of 'cooler climes' and having to undergo thumb surgery;

3. regards the temptation of cooler climes and undergoing thumb surgery as compatible (the visit will have to be put off);

4. believes that recognizing the compatibility of the temptation and the necessity of the surgery will increase the reader's inclination to have positive regard for the claim that the thumb surgery news is true, and is not just an excuse for not visiting.

The third example is also a message from the electronic bulletin board at ISI:

1. The next music day is scheduled for July 21 (Saturday), noon-midnight.

2. I'll post more details later,

3. but this is a good time to reserve the place on your calendar.

The writer of this text wants readers to believe that they should mark their calendars for the next music day. In unit 2, he acknowledges that there is a potential incompatibility between planning for the event and not having more details as to location and specific activities, but he hopes that readers will see that this isn't a real incompatibility, since they are more likely to attend if they can at least refrain from scheduling anything else for that date.

So far, then, we have seen three brief examples of the concessive relation at work in short texts. What insights can we gain by considering concession from this perspective?

First, the definition of concession given above makes explicit that the grammar of clause combining is part of the writer's supply of tools for accomplishing her/his purposes in creating the text. Recognizing this fact resolves the problem alluded to by Jordan (1985) of determining to whom a fact must be surprising in order for the definition of concession to apply.

Focussing on concession in terms of the work that the text is doing for the writer also frees us from the temptation to think of concession in terms of the interpretation of sentences in isolation. Only in terms of its discourse context can we understand how concession is a 'conceding' of something: it concedes the potential incompatibility of two situations in order to forestall an objection that could interfere with the reader's belief of the point the writer wants to make. Looking at sentences in isolation, it is impossible to infer writer purposes, and therefore impossible to determine what relation is exhibited.

Second, to return to König's point about the difficulty of distinguishing between concession and adversative, the text-functional definition I have given allows a clear distinction to be drawn between CONCESSION as a device a writer can use for manipulating readers' beliefs and neutral CONTRAST, which involves no manipulation. A definition of CONTRAST might be the following:

> A pair of text spans are in a relation of CONTRAST if the situations they present are taken to be the same in many respects, different in a few respects, and compared with respect to one or more of these differences.

Here is an example to illustrate the difference between CONCESSION and CONTRAST; this is the abstract introducing a Scientific American article:

1. Animals heal,
2. but trees compartmentalize.
3. They endure a lifetime of injury and infection
4. by setting boundaries that resist the spread of the invading microorganisms.

In this abstract, units 1 and 2 are in a relation of CONTRAST according to the definition just given. It is clear that the definitions we have given of CONTRAST and CONCESSION allow a sharp analytic distinction to be made according to whether the writer can be seen as intending to manipulate the reader's beliefs or not: the definition of CONCESSION includes a component of manipulation, whereas that of CONTRAST does not.

Third, thinking of concession as a discourse-functional relation rather than as strictly an interclausal relation allows us to view CONCESSION independently of any particular markings; thus, as has been noted by König (1985, to appear) and Harris (1986), we find not only that there are many ways of signalling CONCESSION , but also that such 'concessive' morphemes as although, even though, and while don't always mark CONCESSION.

Examples of CONCESSION being marked in more than one way can be seen in our first two text extracts above; the first uses although, the second shows but.

An example of the second situation, where although marks a non-concessive relation can be seen in this paragraph from New Scientist, Aug. 11, 1966, p. 333, cited in Winter (1982:111-112) about a group of Mbuti people who were persuaded to be coached by filmmakers to make dangerous river crossings:

1. They were undoubtedly an obliging people.

2. The famous photograph of the pygmy 'bridge' and the spectacular technique of crossing a river by swinging on a vine from one side to another was taught to the Mbuti 'not without difficulty' by an enterprising moviemaker.

3. The group were able to keep it up for some years

4. and 'obligingly' repeated the act for 'documentary' film units

5. although they preferred to cross the river by wading or by walking over a tree trunk.

6. It was far safer.

The definitions given above of CONCESSION and CONTRAST suggest that this text, despite the connective although, is not an instance of CONCESSION, but is rather an instance of simple CONTRAST, since nothing is being conceded, no potential objections are being answered, no beliefs are being manipulated.

Harris (to appear) observes that 'the notion "concession" is not always explicitly marked by a specific subordinator or the equivalent in a particular language'. He goes on to suggest that 'a conditional marker and/or an adversative co-ordinator will often serve the purpose just as well'. However, my data suggest that in fact the CONCESSION relation may not be marked at all. Here is an example from a memo to members of a linguistics department from the administrative assistant explaining why they can't be reimbursed for off-campus xeroxing:

> 1. Some of you have occasionally given me receipts for Xeroxing done off-campus.
>
> 2. Until now I have never had any trouble getting these reimbursed for you.
>
> 3. Now the Accounting Department is clamping down and enforcing a regulation that they claim has been in effect since July 1976 that all Xeroxing on University accounts must be done through the copy centers on Campus.

The CONCESSION relation between units 2 and 3 is clear: the writer wants her readers to believe that they can't get the reimbursement they have come to expect. Acknowledging the apparent incompatibility between the previous ease with which these payments have been made and the current impossibility of getting them now increases the likelihood that her point will be believed, since it forestalls the objection 'but we have been getting reimbursed with no problems.'

These three examples suggest, then, that if we view CONCESSION in terms

of defining certain connectors or in terms of artificial example sentences, we might miss the functional unity of this relation as a means for a writer to accomplish certain goals whether or not it is explicitly signalled.

I wish to emphasize that this logical independence of form and function does not deny a close relationship between the clause-combining grammar traditionally associated with concession and the functions I have been discussing. What seems to me an appropriate way of regarding this relationship is to see the grammar of clause combining as a grammaticalization of discourse relations (as discussed in Matthiessen and Thompson (to appear)); thus forms such as although and but are often, but need not be, associated with the discourse function of CONCESSION. As Du Bois (1985) has put it, 'grammars do best what people do most'.

5. Conclusion

In this paper, I have tried to show that viewing CONCESSION as a discourse relation rather than in terms of the traditional semantic characterization of 'surprise' can provide a fresh perspective on the way writers and readers actually use CONCESSION in English.

Notes

1. I wish to thank Cecilia Ford, Barbara Fox, Martin Harris, Michael Hoey, Michael Jordan, Ekkehard König, William Mann, and Christian Matthiessen for much stimulating discussion of the ideas in this paper. I of course take full responsibility for the interpretation that I have given to their suggestions.

2. I will refer to 'writer' in this paper because I am restricting myself to written language here; the claims I am making are equally applicable to speakers as well.

3. Literature discussing discourse relations includes Beekman and Callow (1974), Crothers (1979), Grimes (1975), Halliday and Hasan (1976), Hobbs (1979), (to appear), Longacre (1976), (1983), Mann and Thompson (1985), (1986), (to appear), Matthiessen and Thompson (to appear), Martin (1983), McKeown (1982), Meyer (1975), and Thompson and Mann (to appear). For a detailed discussion of a theory of such discourse relations, see Mann and Thompson (to appear).

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Adverbs, Polysemy, and Compositional Semantics

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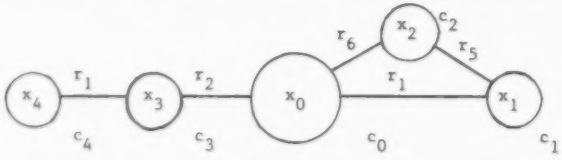
Much of recent work on lexical semantics, e.g. Sweetser (1986) and Welsh (1983), has taken an approach to word meaning which recognizes the existence of widespread polysemy. This view emphasizes the systematic nature of the relations between polysemous senses of a word; in particular, polysemous senses are frequently linked by certain common, pervasive relations. Perhaps the more common view in formal linguistics, in contrast to this polysemy approach, is that different but related senses of a word are to be treated as 'homonyms', formally on par with standard homonyms such as bank (of a river) and bank (for money). This move makes an account of compositionality easier. Theories which appeal to polysemy and prototype semantics (and in practice the two often go together) face particular problems in accounting for compositionality (see, e.g., Osherson and Smith (1981), and opposing viewpoints in Zadeh (1983) and Welsh (1986)).

Both sides base their theories primarily on the meanings of such words as nouns and adjectives. My purpose in this paper is to examine certain cases of polysemy in adverbs, and see how they fare with respect to semantic composition. There is a good reason for wanting to do this: adverbs have a wider range of compositional possibilities than other content classes, regularly modifying sentences, VP's, verbs, adjectives, other adverbs, prepositional phrases, and NP's (cf. McConnell-Ginet (1982), Ernst (1984)). In some cases the very same adverb has all of these functions. Adverbs therefore provide a wide range of test cases for examining the proper formulation of word meanings, in such a way as to account for the facts of semantic composition.

I will assume the general framework of Bartsch (1984), which provides a way to discuss polysemous senses in formal terms. She represents the meaning of a polysemous word schematically as in (1):

(1)



In (1), X_i stands for J (A, c_i), which is a set of properties and relationships manifested in the context c_i, i.e. a function from words to contexts where the word A can be used. Bartsch then defines the meaning of a word as in (2):

(2) Meaning of a word: $M(A) = \langle \{ J(A, c_i) \mid c_i \in C \}, R \rangle$, plus derivational rule: For every c_i of use of A there is at least a c_j such that $J(A,c_i) = R(J(A,c_j), c_i)$.

The derivational rule says essentially that there is another context of use with related (similar) properties; this is represented in (1) by the lines connecting the circles.

Bartsch gives as an example the case of run, where the kernel sense applies to a person, and includes the features 'change one's position', 'by moving', 'in an upright position', and 'by stepping on the ground with at most one foot at the same time and alternatingly.' For the sense found in a sentence like Buses run on Sundays, the latter two features are absent; for The water is running, again only the first two are present but others are added relating to the characteristic motion of liquids.

Metaphor and metonymy (which Bartsch focusses on) are among the common types of relations between senses, but there are others. In one case, Lakoff and Brugman invoke the relationship connecting a moving point and the line it traces, which accounts (for example) for the polysemy of extend in (3):

(3) a. It extended a tentacle and groped along the seabed.
b. This road extends all the way to West Cupcake.

A similar relationship underlies the two senses of evenly in (4), where there are discrete entities evenly spaced in (a), but a smooth gradation in (b):

(4) a. The violin-maker tapped evenly around the edge of the sounding-board.
b. He was a master at shading his colors evenly from one into the other.

Still other types of relationships are necessary in accounts of prototype-senses, e.g. where an old-style 'pennyfarthing' bicycle or a chicken must be related to prototypical bicycles and birds, respectively (cf. Wierzbicka (1985), Welsh (1988)). In such cases we often must discuss variations of shape, function, and other factors that can somehow be perceived.

With adverbs, it is often necessary to have recourse to somewhat more abstract relationships. In the rest of this paper I will discuss three cases of adverb polysemy, where the required relationships involve human agenthood and communicative intent.

As was mentioned above, adverbs provide useful material for the study of polysemy because they have a wider range of compositional possibilities than other categories. In (5a), for instance, rudely indicates that Dave was rude because he left, irrespective of the way in which he left, and as implicitly opposed to not leaving:

(5) a. Rudely, Dave left.
b. Dave left rudely.

This is an Ad-VP reading, in the terminology of McConnell-Ginet (1982). In (5b), on the other hand, Dave is judged rude because of something about his leaving—perhaps his slamming the door or not saying goodbye. He may have been perfectly polite to leave, per se. This is an Ad-V reading. This dual possibility is standard for such Agent-Oriented adverbs, a group containing cleverly, bravely, stupidly, and many others in addition to rudely.

A similar pattern is found with Evaluative adverbs such as oddly and appropriately:

(6) a. Appropriately, it was a judge who founded the Law School.
b. They acted quite appropriately.

In (6a), appropriately takes the entire rest of the sentence within its scope: the appropriate entity is the situation that it was a judge who founded the Law School. In (6b), parallel to (5b), it is something about the actions designated by the verb that is called appropriate, not that the action was taken per se.

In the (a) sentences, rudely and appropriately differ, in compositional terms, primarily in that when combined with a quantified subject NP, appropriately takes the quantifier in its scope, while rudely is within the quantifier's scope. This is shown by the simplified formulas in 8, corresponding to the sentences in (7):

(7) a. Rudely, everybody left.
b. Appropriately, everybody left.
(8) a. ∀x RUDE (x, x left)
b. APPROPRIATE (∀x (x left))

(RUDE is to be read 'can be judged rude because'.) Thus while rudely is an Ad-VP in (5a) and (7a), appropriately is an Ad-S in (6a) and (7b).[1]

How most adverbs, when not Ad-V's, are either Ad-S or Ad-VP. However, a small number of them are both, and the two compositional possibilities correspond to two polysemous senses. (9-10) are examples:

(9) a. Mercifully, they gave the prisoner five minutes to rest.
b. Mercifully, it was no longer raining with gale-force winds when we were forced from our shelter.
(10) a. Perversely, Alice refused to come along.
b. As they climbed, the cliff perversely tilted at ever more difficult angles.

(9a) can be interpreted where the speaker is attributing mercy to the people who let the prisoner rest. In (9b), however, there is no agent claimed to be merciful; instead, the focus is on the patient who is the beneficiary of the change in weather. In (10), again, some agent (Alice)

is labeled perverse in (a), but in (b) we do not attribute some perverse intention to the cliff. In both (b) sentences the adverb rather signals the effect that some situation has on some patient. Thus we have Ad-S's there; by contrast, in (9a)-(10a), as in (5a) and (7a), there are Ad-VP's.

This compositional distinction is not arbitrary. Agent-Oriented adverbs always involve the agent's control of an event, in the sense that this agent at least has the possibility of not participating (cf. Dillon (1974)). Thus beside cases such as (11), Agent-Oriented adverbs also appear as in (12), where the agent 'acts passively' in allowing something to happen (imagine that gangsters have tied him up and pushed him in, unaware that Clark Kent is also Superman):

(11) Ken wisely moved out of the steamroller's path.
(12) Clark Kent wisely fell all the way to the bottom of the mineshaft to protect his secret identity.

Evaluatives, on the other hand, often focus on the effect a situation has on someone or some thing: luckily, unfortunately, and conveniently, for example. It seems to be precisely those Agent-Oriented adverbs which have a salient role for the patient as well that can also be Evaluatives. Note that this effect shows up equally clearly in the corresponding adjective forms, which share the same core of meaning:

(13) Jill was $\left\{\begin{array}{l} \text{unjust} \\ \text{merciful} \\ \text{perverse} \\ \text{\$clever} \\ \text{\$wise} \end{array}\right\}$ to us.

In (13) it is only those words whose adverbial form can be either Agent-Oriented or Evaluative that allow the phrase to us, which indicates a patient.

Accordingly, it is possible to express the relationship between the two possibilities within a theory of polysemous adverbs. Let us take mercifully as an example, and represent its kernel sense, schematically, as in (14):

(14) $\underline{\text{mercifully}}_{VP}$: $\lambda F \lambda x$[CONTROL(x, F(x)) & CAUSE(F(x), REDUCE (pain))]

In (14) mercifully is an Ad-VP, combining with a predicate F to make a larger predicate. The element 'CONTROL', as mentioned above, is central to Agent-Oriented adverbs; a full representation for mercifully should include much more detail, such as benificent feelings on the part of the agent. The second clause of (14) indicates that the event controlled by the agent causes a 'reduction of pain'—a formulation that, again, is only meant as an approximation for the effect of the event on the patient. (9a) can now be represented by (15):

(15) CONTROL (b, GIVE (b, prisoner, 5 min)) &
CAUSE (GIVE (b, prisoner, 5 min), REDUCE (pain))

The referents of they consciously do something merciful, i.e. as agents they control an event which causes a lessening of some sort of pain. But for (9b) there is no such agent. Therefore there is no CONTROL clause, and the sentence can be represented as in (16):

(16) CAUSE ((~STILL (RAIN...), REDUCE (Pain))

Underlying (16) is the Ad-S, Evaluative sense of mercifully shown in (17):

(17) mercifully_e: λP (CAUSE (P, REDUCE (Pain)))

The two polysemous senses shown in (14) and (17) are equally applicable to the ambiguous sentence (18), which can be understood either with the referent of they making conscious decisions to leave (see (19a)), or with the focus on the patient's relief at their departure (see (19b)):

(18) Mercifully, they all left early.
(19) a. ∀x (CONTROL (x, LEAVE-EARLY (x)) &
CAUSE (LEAVE-EARLY (x), REDUCE (Pain)))
b. CAUSE (∀x (LEAVE-EARLY (x)), REDUCE (Pain))

Given this analysis, we can say that the kernel sense of polysemous adverbs like mercifully—that is, X_0 in (1)—has the form of (14), and that r_1, deriving or relating X_1, is (20):

(20) r_1 for 'patient-oriented' Agent-Oriented adverbs:
Delete CONTROL (x, F(x)); X_1 is Ad-S.

(I take the second clause of (20) as responsible for changing 'λFλx' in (14) into 'λP' in (17).)

The second case of polysemous adverbs involves significantly. First examine the sentences in (21):

(21) a. Significantly, the treasury was empty the day after the dictator fled.
b. This configuration occurs significantly in the data.

The first sentence has an Ad-S; what is significant is indicated by the sentence following the adverb. In (21b) significantly is a predicate-modifier, an Ad-V, since it is something about the (pattern of) occurrence in the data that is significant, not the fact that occurred. Given a rule such as the one for predicate modification in Ernst (1984), however[2], these need not be considered polysemous senses, just as with rudely in (5). Instead, they share the basic meaning of significantly, which can be paraphrased as 'particularly indicative of P', P being some contextually-determined proposition.

Genuinely polysemous occurrences are illustrated in (22):

(22) a. Jane coughed significantly when Harry started to talk politics.
b. Oswald arched his eyebrow significantly.

In (22) it is not simply the case that the manner of Jane's coughing or Oswald's raising his eyebrow is particularly indicative of something. Rather, there must be a conscious attempt on the part of Jane and Oswald, respectively, to communicate a message. For example, we could imagine a case where something about the sound of Jane's cough strongly indicated to a doctor that she had bronchitis, but Jane coughed significantly cannot be used to describe such a situation. Therefore (22) must have a sense different from but related to the one illustrated in (21): 'Be a deliberate/intentional attempt to be particularly indicative of P'.

The relevant aspects of (21b) and (22b) are represented, respectively, in (23a-b); m(X) (an abbreviation for a more detailed formalism) can be taken as representing 'a manner of X-ing', where X is the predicate:

(23) a. INDICATIVE (m(occur), P)
b. INTEND (Oswald, (INDICATIVE (m(raise-eyebrow), P)))

Thus the manner (i.e. pattern) of the configuration's occurrence is especially indicative of something in (23a), while in (23b) Oswald makes a deliberate, intentional attempt to have the manner of eyebrow-raising be indicative of something--that is, to communicate something. The relation between X_0 in (23a) and X_1 in (23b), r_1, is therefore:

(24) r_1 for significantly:
INTEND (a, Q), where a is the agent and Q is X_0; X_1 is Ad-V.

Note that INTEND in (24) is not exactly the same thing as CONTROL in the case of mercifully, although they are similar; the former entails the latter, and furthermore requires a more active participation than the latter.

The final case involves the adverb frankly, which is not as typical an example of polysemy as in the two cases examined above. It is, however, a good candidate for a prototype analysis, and in showing how it can be handled in the same model assumed here, I would like to suggest that polysemy and prototype phenomena can be seen as aspects of the same thing. Of particular interest is the fact that one type of occurrence of this adverb always has its prototypical meaning, while when it combines in a different way compositionally it may have a less prototypical reading.

I wish to argue that there are (at least) two important components to the meaning of frankly:

(25) a. willingness to COMMUNICATE
b. content of communication is something one might want to hide

These are illustrated in (26):

(26) a. He spoke frankly with us.
b. They looked each other up and down frankly.
c. ?Ellen looked over at him frankly.
d. They moved their {*arms / pelvises} frankly.
e. *They dug up the treasure frankly.

(26a) is a prototypical case, where it is clearly an act of communication, and the context may easily be such that there is some reason to hide something. (26b) is somewhat less prototypical. Imagine a man and a woman who meet each other for the first time and are attracted to each other; there might be a reason to hide their attraction, but this context is not quite so clearly a matter of communication. In (26c) once again the element of communication is not salient, and also there is less contextual support for wanting to hide some information, so the sentence is even less prototypical than (26b). Note that it really is a matter of salience of the communication context:

(27) Ellen looked over at him {significantly / pointedly / ?frankly}.

In (27) the relative acceptability of significantly and pointedly indicates that frankly requires a relatively strong context in this regard, while the others do not. I find (26d) with pelvises about as good (prototypical) as (26b). But with arms, the contextual need to hide something is totally absent, rendering it much worse. Finally, (26e) shows a case where the possible need to hide information is salient, but the element of communication, in contrast to the contexts in (a-d), is totally absent. Compare openly substituted for frankly in (26e): this word has only the element of hiding something, not the requirement for saliency of communication, and is perfectly acceptable here as a result.

Suppose we consider the non-prototypical cases as being related to the prototype by a generalized relation of the form:

(28) Generalized r_1 for prototypes:
Reduced salience of F, where F is some feature of X_0.

Of course, (28) does not answer many of the interesting and relevant questions about prototypes here (for example, which features can be reduced in salience under what conditions and combinations and still allow an acceptable usage), but it will do for our present purposes. Now examine a case where frankly functions not as an Ad-V, as above, but as a Discourse-Oriented adverb (sometimes called 'Pragmatic' or 'Performative' adverb):

(29) Frankly, it's a stupid idea.

There is a rule of composition for such readings, which covers frankly and other adverbs like roughly, briefly, and honestly, requiring

the adverb to indicate something about the way the information of the following sentence is presented to the addressee (cf. McConnell-Ginet (1982), Ernst (1984)). In such cases, of course, the communication content is necessary and salient, and this clearly part of the motivation for the performative hypothesis, where (for example) (29) would be derived from (30).

(30) I say frankly (that) it's a stupid idea.

Given the model of polysemy assumed here, the fact that such Discourse-Oriented readings always involve prototypical cases of frankly falls out from the relation in (28): r_1 will always be incompatible with the requirements of the compositional rule. This is of course not the case with the Ad-V readings of (26), so nonprototypical cases may occur. Thus the prototype-as-polysemy model allows us to state this asymmetry under the two compositional possibilities.

In addition to the three cases of polysemy discussed here, there are other instances of apparently polysemous adverbs relevant to the interaction of word meanings and compositional rules. Just to mention two examples, in (31) we see logically as an Ad-S (in (a)) and an Ad-V (in (b)):

(31) a. Logically, this analysis is incoherent.
b. He acted very logically.

Although such Domain adverbs often have such dual uses analyzable as having the same sense, the fact that logically is gradable in (31b) but nongradable in (31a) indicates the need to explore a polysemy analysis for examples like this one. And parallel to the Agent-Oriented/Evaluative connection for mercifully, there is a small number of adverbs like sadly in (32) which can be either Evaluative (32a) or Mental-Attitude adverbs (32b):

(32) a. Sadly, his reign ended after only twelve years.
b. Sadly, she turned away from the empty shelves.

Such cases indicate that there is more to be learned from adverb polysemy.

In conclusion, I have shown that a number of cases of adverb polysemy can be handled under a model where specific relations between polysemous senses are posited. The relations include both 'content' factors--CONTROL, INTEND, salience of communication content--and compositional information, so that the correct sense enters into the correct combinations. Moreover, it seems as though instances of prototype meaning may be accounted for as a subcase of polysemy.

The cases examined here are by no means the only ones, and a wider investigation should shed more light on word meanings and their interaction with rules of composition. In particular, it should be illuminating to find out what sorts of relations exist between polysemous senses; besides the spatially-based relations invoked in much recent work (e.g. Lakoff and Johnson (1980), Brugmann (1981)) we find here relations

rooted in human intention. Finally, we may hope that these investigations will tell us why such 'content' features as INTEND are linked to certain compositional possibilities; surely, it is not an accident that an adverb making crucial reference to a thinking, individual agent is within the scope of a quantifier (so that each individual controls his action; cf. (19a)) while those which focus on the effect of some event on a patient are not (cf. (19b)). In this way we may hope to ultimately connect logical form to human experience.

Notes

1. I argue in Ernst (1984) that the distinction between Ad-V and either Ad-VP or Ad-S need not and should not be lexically specified, but is instead predictable from other factors and can be abstracted out as a general rule of semantic composition. This will not affect the point at hand, however, which concerns the Ad-VP/Ad-S distinction.

2. The actual formalization of this rule in Ernst (1984) is faulty, and a revised form of the rule is presupposed in (23) below, although full justification of this version is as yet unpublished. Details of formalization are not important for the point under discussion here.

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Lake Miwok Naturalization of Borrowed Phonemes

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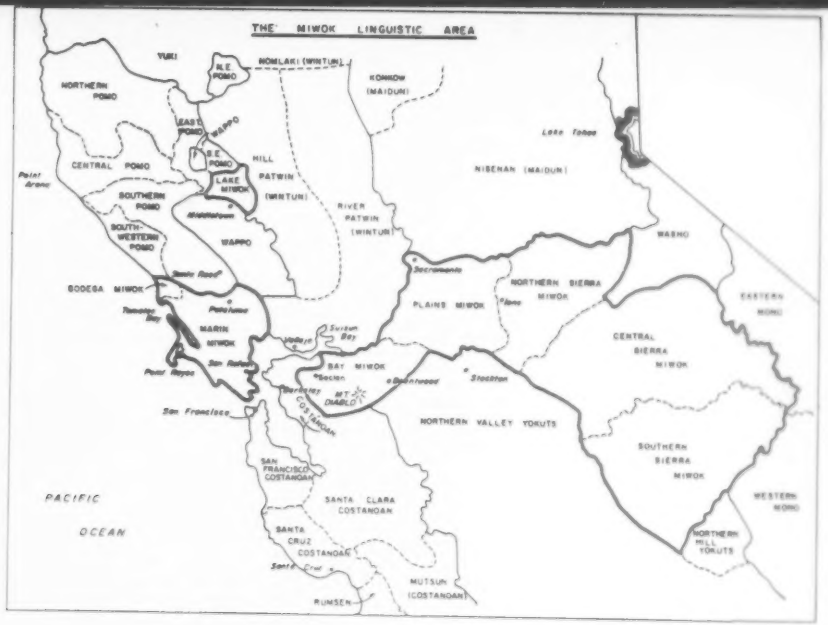
Lake Miwok is a California Indian language formerly spoken in a small area south of Clear Lake, about 95 miles north of San Francisco. It is closely related to Coast Miwok, once the language of the Marin Peninsula north to Bodega Bay, and more distantly related to Eastern Miwok, formerly spoken on the western slopes of the Sierra Nevada Mountains and a stretch of territory extending across the northern portion of the San Joaquin Valley (see map). The Miwok family is in turn related to the Costanoan languages, once spoken from San Francisco south to Big Sur.

On the basis of lexical items, structural similarities, and sound correspondences, the Miwok languages may be grouped as follows (Broadbent and Callaghan 1960, Callaghan 1971):

I. Eastern Miwok (Mie)
   A. Sierra Miwok (Mis)
      1. Northern Sierra Miwok (Mins)
      2. Central Sierra Miwok (Mics)
      3. Southern Sierra Miwok (Miss)
   B. Plains Miwok (Mip)
   C. Saclan (Misac)
II. Western Miwok (Miw)
   A. Coast Miwok (Mic). Coast Miwok was probably a single language with various dialects.
      1. Bodega Miwok (Mib)
      2. Marin Miwok (Mim)
   B. Lake Miwok (Mil)

Modern recordings exist for six Miwok languages; Southern Sierra Miwok (Broadbent 1964 and my field notes), Central Sierra Miwok (Freeland and Broadbent 1960 and my field notes), Northern Sierra Miwok (Callaghan forthcoming), Plains Miwok (Callaghan 1984), Lake Miwok (Callaghan 1965) and Bodega Miwok (Callaghan 1970). Saclan, now extinct, is known from a short list of words and phrases taken by Fray Felipe Arroyo de la Cuesta in 1821 (Beeler 1955).

We see that Lake Miwok was geographically isolated from its nearest relatives, although speakers of Lake and Coast Miwok were in frequent contact. This isolation may be relatively recent. Kenneth Whistler (1977) argues for Patwin intrusion into the lower Sacramento Valley and adjacent foothills, basing his conclusions on Patwin plant terms of Miwok provenience. James Bennyhoff (personal communication) has presented archaeological evidence for recent expansion of Wappo territory. In addition, Lake Miwok is bounded by Eastern and Southeastern Pomo, and it is close to Southern Pomo territory.


THE MIWOK LINGUISTIC AREA
YUKI
N.E. POMO
NORTHERN POMO
EAST POMO
WAPPO
CENTRAL POMO
S.E. POMO
HILL PATWIN (WINTUN)
LAKE MIWOK
SOUTHERN POMO
SOUTH-WESTERN POMO
WAPPO
RIVER PATWIN (WINTUN)
KOMKOW (MAIDUN)
NISENAN (MAIDUN)
Lake Tahoe
Point Arena
BODEGA MIWOK
MARIN MIWOK
PLAINS MIWOK
BAY MIWOK
NORTHERN SIERRA MIWOK
CENTRAL SIERRA MIWOK
SOUTHERN SIERRA MIWOK
WASHO
EASTERN MONO
WESTERN MONO
NORTHERN HILL YOKUTS
NORTHERN VALLEY YOKUTS
MT. DIABLO
Sacramento
Stockton
San Francisco
SAN FRANCISCO COSTANOAN
SANTA CLARA COSTANOAN
SANTA CRUZ COSTANOAN
MUTSUN (COSTANOAN)
RUMSEN
PACIFIC
OCEAN

The phonemic system of Proto Miwok probably configurated as follows (Callaghan 1971):

p	t	ṭ	č	k	ʔ	i	y[ɨ]	u
	s	ṣ			h	e		o
							a	
m	n							
w	l		j[y]			length (•)		

Only Central Sierra Miwok retains both /s/ and /ṣ/. The Sierra Miwok languages have added /ŋ/, and Plains Miwok has added /ə/.[1] Coast Miwok has lost /y/, and Plains Miwok has neutralized the contrast between /t/ and /ṭ/. Otherwise, daughter languages other than Lake Miwok have retained the phonemic system of the parent language. (By convention, [č] is written /c/ in these languages.)

Lake Miwok has also lost Proto Miwok /y/. But the Lake Miwok consonantal system is extremely complex.

p	t	ṭ		k	ʔ
pʰ	tʰ	ṭʰ		kʰ	
p̓	t̓	ṭ̓		k̓	
b	d				
	c[ts]	č			
	c̓[ts]	č̓	ǩ		
	s	ṣ		ɬ	h
m	n				
w	l	r	j[y]		

/s/, /r/, and /č/ are extremely rare, except in loan words from Spanish. /č/ varies with /c/ or /č̓/ in other words, and /č̓/ also varies with /č/ in those few items in which it occurs.

The Pomo languages, Wappo, and Patwin all have multiple series of stops. In addition, Patwin has /ɬ/ and /ǩ/. An early comparison of Lake Miwok words containing non-plain stops or affricates, or ɬ, revealed that about 30 percent closely resembled corresponding words in neighboring languages. Moreover, these Lake Miwok words rarely had a Miwok etymology. Consequently I concluded that Lake Miwok had undergone massive phonemic borrowing as a result of loan words from nearby languages (Callaghan 1964). Additional evidence came from the fact that the aberrant phonemes do not occur in Lake Miwok affixes (except for a few reduplicating suffixes and free variants of the objective case), and they are largely absent from core vocabulary. Therefore, I was confident that additional research would yield sources for the remaining 70 percent of the problematic items.

Chart 1 *p > Mil p̓ ~ p / _ V+back

English	Sierra Miwok	Plains Miwok	Coast Miwok	Lake Miwok	Other[2]
to kiss				p̓út-ka-ṭi, p̓út-ak	Pse p̓uṭki 'a kiss'
woman			Mim pōṭciṣ SB 'old woman'	p̓óc·i ~ póc·i	Wph p̓okita Wph p̓oksin 'sister-in-law'
rash				p̓óclo-ṭi ~ póclo-ṭi	Csjb po·čor 'a sore' Waw pótlo? 'syphilis'
to cut open, cut off	Mics po?-wa-, po?-la-			p̓óc·a ~ p̓óc·a	
to blow on, blow out			Mib púc·u-ṭi	p̓úc·a-ṭi ~ púc·a-ṭi	
to be blowing, blow slowly			Mib pú·cu	p̓ú·ca ~ pú·ca	
wild onion			Mib, Mim putcu IK	p̓ú·cu ~ pú·cu p̓ú·du	Wph pu·rway
snake				p̓ódwaj	Wph porwan
to break (a branch) off	Mics pej·y-			p̓ój-ku-ṭi, p̓ój?a	
to smoke (tobacco)	PMis *pa?my-	pa?my-		p̓óm·a ~ p̓óm?a 'to puff, suck (a pipe)'	

Chart 2 Lake Miwok d, ɬ, -n·e

English	Sierra Miwok	Plains Miwok	Coast Miwok	Lake Miwok	Other
to tear				ʔcúd-ka-ṭi	Wph čura
to rub against	PMis *liṭ-ja-			líd·a	
slick, slippery	PMis *liṭ·a-ṭa-				
to skin (an animal)	PMis *luṭ·u- ~ *lu·ṭú-	lut·u-		lú·doj 'to cut into strips'	
big, much, many	PMis *ʔyṭ·y-			ʔudí·, ʔúd·i 'great, huge' ʔú·di 'eldest'	
Sulphur Banks				móɬ	Wpcc moɬ 'willow, bay leaves'
to break off	Miss tes-ku-			déɬ-ku-ṭi	
to break off, break up			Mib kéṣ·a, kéṣ-ku-ṭi	kéɬ·a	
to hang down	Mins juh·u-c·u- < *juṣ·u-			jóɬ-ka-ṭi 'to hang out'	
to have diarrhea	PMis *čul-ka-			cóɬ-ka-ṭi	
to be hanging			Mib jó·ke-ṭe ~ jó·ke-ṭi	jokó-n·e ~ jokó-n·e	
to sit down, land (bird, insect)	PMis *ṭo?-ṇe-			taká-n·e 'to land'	
to squat	Miss waṭa·-t- 'to straddle'			waṭá-n·e	

Chart 3 Onomatopoeia and Sound Symbolism

English	Sierra Miwok	Plains Miwok	Coast Miwok	Lake Miwok	Other
fish trap				búkʰal	Pe buˑxál < *buhqʰál
to belch				bák-ṣe-ṭi	Waw páke?
to bubble	Mins poklu-			bókbok-oṣi	
	Miss puṭˑa-			bótbot-oṣi 'to foam and pop'	
to growl (intestines)	Miss kol-ka-			ʔkowóˑlod-oṣi	Wph ʔkoˑworo
to squeak (mouse, door)	Mins ciliṭ-ˑiṭˑy-			ʔcitíˑt-iṣi	Waw ʔcíṭiṭisi?
to squirt	Mins ciṭiˑṭ-			ʔcíd-ka-ṭi	Wph ʔtid-
				ʔcidíˑt-it-iṣi	
to wink	PMis *čituk-nu- 'to wink at someone'	cituk-nu- 'to wink at someone'		ʔcít-ka-ṭi, ʔcít-ak	Waw ʔciw-
to drip	Mins cot-ka-			ʔcakáˑt-at-aṣi	
to point		kiˑca-		kíˑci	
index finger		kiˑca-nˑa-		kiʔcíˑn-i	
tooth	PMis *kytˑy-	kəˑt	Mib kút	kút	
to chatter (teeth)	Miss kyṭyṭ-ˑyṭˑy-			kuṭúˑkuṭu - kúṭúˑkuṭu 'to gnaw noisely'	
to roll around			Mib púcˑel 'to roll over'	puʔcéˑl-eṣi	
to crush, mash	PMis *picak			pʰíʔcak	
cocoon rattle	PMis *ṣokoṣˑa-	sokosˑa-		ṣokóṣˑo	
to flip (fish)			Mib típ-le-ṭi	ʔípʔip-iṣi	

Additional research did not shake the hypothesis of massive borrowing, but it did render the situation much more complex. In some cases, new loan words were clearly identified. Mil lébleb 'solar plexus' is most probably from Wph lebleb 'meat under ribs'. But in other cases, an increasing number of resemblant forms emerged in other Miwok languages. Sometimes there was an embarrassment of riches. Mil čít-ka-ṭi 'to wink' was thought to derive from Waw číẅ- 'wink'. But it acquired a much more convincing Miwok etymology in view of PMie *čituk-nu- 'to wink at someone'. Some of the new phonemes have become full-fledged participants in Lake Miwok sound developments; i.e. they have become naturalized, to borrow a term from the biologists. I will analyze a few instances of this phenomenon.

Chart 1 lists some examples of Mil /ṗ/ in initial position. This phoneme entered the Lake Miwok corpus through such words as pút-ka-ṭi 'to kiss (once)', which is presumably from Pse puṭki 'a kiss', and for which there are no known Miwok cognates. It is in the process of spreading to all words in which /oc/ follows. The spread to ṗóc·i - póc·i 'woman' was doubtless encouraged by Wph pokita 'woman' and Wph poksin 'sister-in-law'. Mil ṗóclo-ṭi - póclo-ṭi 'rash, to break out' has an apparent remote cognate, Csjb pó·čor 'a sore'. The relationship here of Waw pótlo? 'syphilis' is uncertain.

The new phoneme usually spread by analogy to other members of a derivational paradigm; hence pút-ak 'to kiss (several times)' and ṗó·c-ka-ṭi 'to cut one piece'. The sound development Pse /ṭ/ > Mil /t/ in ṗút-ka-ṭi 'to kiss (once)' is also expected, since two glottalized stops in the same stem are rare, and Mil /ṭ/ is rare before -ka- 'semi-accidental' or -ku- 'deliberate'.

Mil /ṗ/ is also in the process of spreading to words with a following /u(·)c/, but the spread is incomplete, since there are still items such as púci 'to suck, nurse', which do not participate. Mil /ṗ/ has already spread to the two words with a back vowel followed by /d/, ṗú·du 'a plant like garlic' and ṗódwaj 'snake'. Both are probably loans from Hill Patwin.

The final examples; Mil ṗój-ku-ṭi 'to cut off (branch, pine cone)', Mil ṗój?a 'to cut (pine cones) off', and Mil ṗóm·a - ṗóm?a 'to puff several times, suck a pipe'; represent a further spread of Mil initial /ṗ/, this time to words with a back vowel followed by a sonorant. The spread may have been occasioned by the intrusive glottal stop in ṗój?a and ṗóm?a.

/-d-/ entered Lake Miwok via such words as ćúd-ka-ṭi 'to tear', probably from Wph čura 'to tear' (see Chart 2). The sets 'to rub against' (plus 'slick, slippery'), 'to skin (an animal)', and 'big, much, many' argue for the sound change PMi *-ṭ·- > Mil -d(·)- after high vowels.

Mil /ł/ occurs in some loan words from Paṭwin, such as łúkaj - łúkaj 'rabbit blanket', probably from Wph łukay 'rabbit blanket'.

Mil mółł 'Sulphur Banks' may be somehow connected with Wpcc moł 'willow, bay leaves', which might account for an instance of Mil /ł/ in non-initial position.

In fact, Mil /ł/ is common as the second consonant in CVC-stems before -ka- 'semi-accidental' and -ku- 'deliberate', whereas underlying /s/ does not occur in this position in the analyzed Lake Miwok corpus, except in wéł-ka-ṭi - wés-ka-ṭi 'to chip, be chipped' and related constructions. These facts suggest that some instances of Mil /ł/ in this position may derive from PMiw *s, the reflex of both PMi *s and PMi *š. Support for this hypothesis comes from Miss ṭes-ku- 'to break off a piece', an apparent cognate with Mil déł-ku-ṭi 'to break off (a branch), dislocate (a finger)'; and Mins juḥ·u-c·u- 'to hang down', whose stem is probably cognate with Mil jół- in jół-ka-ṭi 'to hang out, protrude'. In both cases, /ł/ has been generalized to other members of the Lake Miwok paradigm.

déł-ka-ṭi 'to pull something off, be out of joint'

déł-ṭe-ṭi 'to droop, bend over, be out of joint'

déł-uk 'to break (twigs) off, dislocate (finger)'

jół-ṭe-ṭi 'to sag, be limp'

Mil kéł·a 'to break or chip bark off all around a tree' is not currently part of such a derivational paradigm, but Mib kés-ku-ṭi 'to break up (table or chair)' argues for such a paradigm in the past, which would explain another instance of Mil /ł/.

Mil cół-ka-ṭi 'to have diarrhea' is harder to explain, since Mil /ł/ is not an expected reflex of PMi *l in this position. Possibly there was analogy with Mil jół-ka-ṭi 'to hang out, protrude'.

The last three examples may represent a morphologically conditioned sound change. Verbal stems of the canon CVCV- glottalize a medial stop before -n·e 'intransitive'. (The verbs in question are all verbs of position.) In these instances as well, the glottalization has spread to related words, i.e. Mil jóḳap 'to hang (a person)' and Mil wá·ṭi 'to spread one's legs when lying down'.

Chart 3 includes some examples that may involve onomatopoeia and sound symbolism. Mil /b/ is rare. It probably entered the Lake Miwok inventory through a few loan words, such as Mil búkʰal 'fish trap', most likely from Pe bu·xál 'fish trap' < *buhqʰál (McLendon 1973: 66-7). It has spread to Mil bókbok-osi 'to boil, bubble' and Mil bótbot-osi 'to foam and pop like acorn mush', both of which are probably of Miwok origin. It may have spread to a loan word, if Mil bák-se-ṭi is indeed from Waw páke? 'to belch'.

The remaining items are apparent instances of sound symbolism, where glottalization has become associated with small, quick, often semi-accidental action. Mil kowó·lod-osi 'to growl (intestines)' and Mil citi·t-isi 'to squeak' are probable loan words from Hill Patwin and Wappo respectively, but their entry into Lake Miwok may have been facilitated by the existence of similar Miwok stems. The glottalization in Lake Miwok words for 'squirt' and 'wink' (as well as the -d- in words for 'squirt') can be explained by the influence of similar stems in Hill Patwin and Wappo. Once glottalization had become established, it began spreading to semantically similar items. The phenomenon is still too sporadic to be predictable.

To summarize, Lake Miwok has probably been isolated from its nearest relatives for centuries. It has undergone massive phonemic borrowing through loan words from neighboring languages, which entered as a result of intermarriage and bilingualism. They spread to some native Miwok words through analogy. As they came to be fully accepted into the language, some of them started to participate in conditioned sound change, sound symbolism, and onomatopoeia.

Notes

1. Saclan might also have had /ə/ and both /s/ and /s̤/.

2. The following are additional abbreviations: IK 'Isabel Kelly', SB 'S. A. Barrett' (both from Callaghan and Bond 1972), PMis 'Proto Sierra Miwok', PMie 'Proto Eastern Miwok', Csjb 'Mutsun' (San Juan Bautista Costanoan, from John P. Harrington's field notes), Pse 'Southeastern Pomo' (from George Grekoff's field notes), Wph 'Hill Patwin' and Wpcc 'Cache Creek Patwin' (both from Donald Ultan's field notes), Pe 'Eastern Pomo' (from McLendon 1973), and Waw 'Western Wappo' (from Sawyer 1965).

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What is KĀRAKA?
A probe into PĀṆINI's analytical procedure

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Abstract

Nominal and verbal stems are found related variously in syntactic constructions. These relationships may be distinguished as KĀRAKA and non-KĀRAKA. Nominal stems, 'things' indicated by which help 'actualization' of action denoted by verbal stems, are called KĀRAKA. The rest are non-KĀRAKA.

In this paper we discuss what KĀRAKA is; why there are only six varieties of it; why nominal stems are labelled as KĀRAKA and what analytical procedure presumably PĀṆINI follows in determining KĀRAKA distinctions.

1. To describe structural patterns in a language, a linguist has to have available to him appropriate and adequate linguistic data. He does not dream his grammar; nor does it dawn on him unobtrusively in his moments of contemplation. His findings are always and necessarily based on linguistic facts gathered from field, sifted and analysed; usually more than once. He may need to replenish his data time and again before he can be reasonably sure of structural patterns. One can comprehend and appreciate structural description of a language better, if he has access to the data on which a linguist bases his formulations.

In our study of PĀṆINI we are handicapped by lack of data recorded in his grammar. Here we are presented with final products of his efforts, a body of structural statements. However an attempt can be made to reconstruct comparable data from linguistic elements mentioned in his statements, explicitly or implicitly, and from various illustrative examples handed down to us by his ancient commentators. Supportive illustrations may be gleaned from ancient literature, though not an easy task to undertake.

Here we propose to study his treatment of KĀRAKAS, a very significant segment of his grammar relating to syntactic structures. It is our attempt to figure out what KĀRAKA signifies, a term not defined formally by him; to make clear why there are only six varieties of it; to explain why nominal stems are designated as KĀRAKA and to work out what analytical procedure he follows in determining various KĀRAKAS.

It is a truism that each structural statement is intended to explicate certain linguistic data. Our endeavor here is, thus, to gather comparable data to serve as premises to understand formulation and meanings of these statements. We try to work our way backwards.

2. What is KĀRAKA?

The term KĀRAKA is used in PĀṆINI as a technical term (cf 237; 3319, 5442 etc.). It is introduced in 1423. Under it are described six varieties of it in 1424-55.

We learn from this description that nominal stems cooccurring with verbal stems as specified here are KĀRAKAS. Consider, for instance, the following constructions.

(1) AŚVAḤ DHĀVATI 'The horse runs'.
(2) AŚVAM ĀROHATI 'He mounts the horse'.
(3) AŚVĀYA DHĀSAM DEHI 'Give some fodder to the horse'.
(4) AŚVENA GRĀMAM YĀTI 'He goes to the village on a horse'.
(5) AŚVĀT AVAROHATI 'He alights from the horse'.
(6) AŚVE TIṢṬHATI 'He sits on the horse'.

In all these constructions AŚVA 'a horse' is a KĀRAKA of one sort or the other - KARTĀ in (1); KARMA in (2) and so on. But that is a subsequent story. First we have to establish KĀRAKA-hood of AŚVA. What does it mean to say that AŚVA is KĀRAKA? What is the feature shared by AŚVA in all its occurrences in the above constructions? PĀṆINI does not answer this question. He straightway makes use of the term to denote nominal stems such as AŚVA without telling us what distinguishing feature marks it off. Let us try to understand what it means as a technical term.

One can readily concede that in writing up his grammar PĀṆINI does not work with nominal and verbal stems as mere lexical items, speculating of syntacitc relations between them. Bare stems in isolation do not contract syntactic relations. To be related structurally, lexical items have to be constituents of some linguistic constructions as found in actual use among members of a speech community. PĀṆINI's account of KĀRAKAS is, thus, securely based on systematic analysis of real language data. To understand what he means by KĀRAKA, we may examine syntactic behaviour of nominal and

verbal stems in linguistic constructions.

Take, for instance, the following sentence.

(7) SARASTĪRE BAKAH EKA-PADENA BUKHENA TIŚTHATI
'There stands with ease on the bank of a pond a crane on one leg'.

Here there are five syntactic units, called PADAS, constituting the utterance. Of these BAKAH; SARASTĪRE; EKA-PADENA and BUKHENA are nominal forms. These are not related among themselves. However all of these are related directly with the verbal form TIŚTHATI. Their relationships with the verbal form differ among themselves significantly. We may examine these a little more closely.

In the first place, 'performance' of the action of 'standing' cannot be conceived without its being related to BAKAH; SARASTĪRE, and EKA-PADENA which respectively answer the questions: who stands?; where does it stand? and with what means does it stand? Mention of all of these expressions, explicitly or implicitly, is required to constitute the construction. 'Things' denoted by these forms help 'actualize' performance of action of 'standing' indicated by the verbal form. Linguistic relationship of such nominal forms with the verbal one is unique in as much as without their association action denoted by the verb form does not get accomplished at all.

On the other hand, in contradistinction to this type of relationship, consider the relationship of BUKHENA, another nominal form, with TIŚTHATI. The nominal form here simply tells us about 'the manner' the act is being performed. It answers the question: how does the crane stand? It stands 'at ease; comfortably'. It neither helps nor prevents 'actualization' of performance of the action.

Consider a few more examples.

(8) DEVADATTAH DIVASAM VEDĀN ADHĪTE
'Devadatta studies the Vedas during the day uninterruptedly'.
(9) YAJÑADATTAH ADHYAYANENA KĀŚYĀM VASATI
'Yajnadatta resides in Kashi for the purpose of studying'.
(10) DYUTAKĀRAH MŪTRAPADENA GRHĀT NIRAGACCHAT
'The gambler slipped away from home under the pretext of urinating'

In (8) DEVADATTAH and VEDĀN related to ADHĪTE tell us respectively who studies and what. Performance of the 'act of studying' cannot be imagined in its entirety without their mention. The expression DIVASAM gives information as to how the act of studying proceeds - the whole day. The action continues even if this expression is not used. Again in (9)

YAJNADATTAH and KĀŚYĀM denote 'performer' and 'locale' of the action. Successful performance of action entails their participation. The expression ADHYAYANENA tells of the 'purpose' of residing as 'studying'. It has little to do with the accomplishment of the action. Likewise in (10) the nominal forms DYUTAKĀRAH and ŌRMĀT as related with NIRAPAICCHAT tell us respectively of 'who' slipped away and 'from where', while MŪTRAPADENA, on the other hand, speaks of the 'reason' of slipping away. Obviously it does not contribute anything toward consummation of the action.

Here two types of relationships may be distinguished between nominal and verbal forms: one, where nominal forms help 'actualize' performance of action denoted by verbal forms and two, where nominal forms have no such roles to play. The former type may be called KĀRAKA and the latter as non-KĀRAKA for want of any other name. Formal distinctions in inflected nominal forms imply that KĀRAKA relations are of different types.

3. Role of inflection in KĀRAKA relations

In our discussion above we have proceeded on the assumption that syntactic relations obtain between nominal and verbal inflected forms. This proposition needs to be examined further. An inflected form is constituted of two elements, namely stem and inflectional suffix. We may look into their respective roles in determining syntactic relations.

Let us go back to the example in (7). We may consider the verbal form first. It consists of STHĀ 'to stand', a verbal root and the suffix TI denoting 3rd person singular, active voice and present tense. The verbal form may be replaced by any one of the forms such as ATIŚTHAT, ASTHĀT, TAŚTHAU all meaning 'it stood'; STHĀSYATI 'it will stand', TIŚTHET 'it may stand' etc. We find that such replacements do not bring about any corresponding reallignment in syntactic relations with nominal forms. The relationships remain intact.

Now consider change of VIBHAKTI suffixes in nominal forms. For instance, if SARAṬĪRE is replaced by any such form as SARAṬĪRAM, SARAṬĪRENA etc. made from the stem SARAṬĪRA, its relation with TIŚTHATI is snapped altogether. Likewise any change of VIBHAKTI in RAKAH etc. results in disruption of relationship with the verbal form. The construction itself becomes incoherent and unacceptable.

Thus syntactic relations, KĀRAKA or non-KĀRAKA, are not dependent on verb inflection. But, on the other hand, any change in nominal VIBHAKTI type either extinguishes the relationship or alters its nature (though in very rare cases). Syntactic relationships are sensitive to nominal suffixes and not to verbal ones.

4. What are structural implications of this?

One obvious implication is that it cannot be maintained that syntactic relations exist between nominal and verbal inflected forms, the position we assumed in our discussion earlier. Nor can it be maintained that these obtain between inflected nominal forms and verbal stems for the simple reason that these two belong to two different levels of linguistic structure. One is a simple stem while the other is more than a stem, an inflected form. The inescapable conclusion, therefore, is that syntactic relations obtain between nominal and verbal stems. The role of nominal inflections, then, is to mark or manifest these relationships.

As a corollary of this it may be held that number of KĀRAKA relations in the language would not exceed that of inflectional types which are just seven. Since one of these, namely the 6th VIBHAKTI primarily denotes non-KĀRAKA relations between nominal stems and marginally such KĀRAKA relations which are indicated by other VIBHAKTI types, only six KĀRAKA relations are postulated by PĀṆINI.

5. Nominal stems are KĀRAKA

There is another important issue which may be considered here. The KĀRAKA is essentially one type of syntactic relationship. It exists between nominal and verbal stems as shown above. For its consummation both are equally important. One is intrigued as to what motivates PĀṆINI to designate this relationship by one of the partners i.e. nominal stems.

It is true that no issue of theoretical nature is involved here. It is simply setting up a sort of convention. In such matters linguist's convenience is supreme. The term KĀRAKA could have been used to denote the type of syntactic relation described above between nominal and verbal stems or it may designate one of the linguistic elements participating in this relationship. PĀṆINI chooses to call nominal stems as KĀRAKA. Presumably the following considerations might have weighed with him.

(i) Nominal stems are considered more than equal partners in KĀRAKA relationship for the reason that these are the carriers of the primary grammatical feature characterizing KĀRAKA relationship, namely VIBHAKTI suffixes. Nominal stems serve as necessary props to which these are tagged.

(ii) Nominal stems are ubiquitous in their pragmatic roles which help 'actualize' different aspects of action denoted by verbal stems cooccurring with them. For instance, AŚVA 'a horse' is capable of exhibiting various roles in relation to different actions denoted by verbal stems

occurring with it in constructions (1-6) given above. Such roles are inconceivable in case of verbal stems.

(iii) Labelling nominal stems participating in the type of syntactic relationship called KĀRAKA as KĀRAKA and those groups of stems that partake in a particular KĀRAKA after the designation of that KĀRAKA, the term KĀRAKA in his system serves as a convenient label for nominal stems in general that participate in syntactic relationship distinguished as KĀRAKA as opposed to one called non-KĀRAKA. And so are APĀDĀNA, KARAṆA etc. for those groups of stems that partake in these particular types. Perhaps there is no other practical way of referring to these stems short of listing them. A few examples are discussed below to show how use of these terms allows him to capture generalizations in structured statements and consequent economy in their formulation.

Consider the statement AKARTARI CA KĀRAKE SAMJÑĀYĀM (3319). THE SUFFIX ṆVUL = A comes after a verbal stem (to form a derivative) in the sense of SAMJÑĀ (= a common noun in specific meaning other than suggested by its etymology) denoting KĀRAKA relations other than KARTĀ (with the stem from which it is derived)'. The statement describes formation of nominal stems from verbal stems by adding the suffix ṆVUL = A to them. The derivatives thus formed denote various KĀRAKA relations vis-a-vis the verbal stems from which these are derived. For instance, DRĀṢA 'a spear' is derived from the verbal stem DRA-AṢ 'to throw'. It is assumed to have KARMA KĀRAKA relation with the stem DRA-AṢ. The hypothetical underlying structure DRĀṢYANTI TAM 'they throw it', posited here, brings out this relationship. The pronominal form TAM in the underlying structure stands for DRĀṢA which is yet to be formed. The point is that PĀṆINI employs the term KĀRKA in his structural statement to denote derivative nominal stems like DRĀṢA which are supposed to indicate various KĀRAKA relations vis-a-vis verbal stems these are derived from.

The nominal stem RĀGA derived from RAÑJ 'to dye' by adding the same suffix denotes KARAṆA relation. It means 'something with which one dyes (something else) i.e. color'. And DRAḌĀTA derived from DRA-ḌAT 'to fall from' denotes APĀDĀNA KĀRAKA meaning 'something from which (one) falls down i.e. a precipice'. Again in KARMAṆYAṆ (321), the term karma denotes nominal stems indicating KARMA KĀRAKA relation with verbal stems occurring with them. The statement is designed to explicate structure of nominal stems like KUMBHA-KĀRA from the underlying artucture like KUMBHAM KAROTI 'He is making a pot'. The suffix AṆ = A is added to KṚ 'to do' which holds KARMA KĀRAKA relation with KUMBHA 'a pot'. A nominal stem KĀRA is thus formed in the meaning 'one who makes'. Now the two constituents representing the underlying structure are compounded obligatorily to give the form KUMBHA-KĀRA 'a maker of pots i.e. a potter'.

Use of KĀRAKA, KARTĀ or KARMA in the statements referred to above stand for groups of nominal stems denoting specific KĀRAKA relations with respective verbal stems. Perhaps one may not be able to think of any other alternative of referring to nominal stems participating in such constructions.

6. Constraints on cooccurrence

This brings us to the question what nominal and verbal stems can go together in a construction denoting particular KĀRAKA relation. These do not occur promiscuously. Take a simple sentence, say, the following.

(11) MṚGAḤ DHĀVATI 'The deer runs'.

It is an acceptable sentence. The stems MṚGA and DHĀV enjoy syntactic compatibility. Each of these may also pair with other stems. MṚGA, for instance, can occur with DRU 'to run'; VI-CAR 'to move about'; KHĀD 'to eat'; PĀ 'to drink' etc., but certainly not with ADHI-I 'to study', MAN 'to think', DIV 'to play gamble', MARD 'to roar', VAD 'to speak' etc. Similarly DHĀV may go with ŚIŚU 'a child', ŚŪKARA 'a boar', VYĀGHRA 'a tiger' etc. but not with STHŪNA 'a pillar', DANTA 'a tooth', ŚŪRYA 'sun', VĀTA 'wind', ŚUKA 'a parrot' etc. Thus in the following consructions the same KĀRAKA relation persists between various pairs of nominal and verbal stem.

(12) MṚGAḤ VICARATI 'The deer moves about'.
(13) MṚGAḤ KHĀDATI 'The deer grazes'.
(14) ŚIŚUḤ DHĀVATI 'The child runs'.
(15) ŚŪKARAḤ DHĀVATI 'The boar runs'.
(16) VYĀGHRAḤ DHĀVATI 'The tiger runs'.

Now consider the following

(17) MṚGAḤ ADHĪTE 'The deer studies'.
(18) MṚGAḤ NARDATI 'The deer roars'.
(19) MṚGAḤ DĪVYATI 'The deer plays dice'.
(20) STHŪṆAḤ DHĀVATI 'The piller runs'.
(21) ŚUKAḤ DHĀVATI 'The parrot runs'.
(22) VĀTAḤ DHĀVATI 'The wind runs'.

Although formally these are structured like the examples under (11), but no KĀRAKA relation is contracted between these pairs. In other words such pairs lack something that does not allow them to go together. Thus such constructions do not get started at all.

It may be said in a general way that only those stems go together which share some feature, grammatical or otherwise, significant syntactically. One has to work out for each pair or group of pairs what such features are.

It may be pointed out that members in a cooccurring pair may share a set of features in one construction and another set in another, thus, denoting more than one KĀRAKA relations. For instance, VṚKṢA 'a tree' and PAT 'to fall' are found to cooccur in the following.

(23) VṚKṢAḤ PATATI 'The tree falls down',
(24) VṚKṢĀT PARṆĀNI PATANTI 'Leaves fall from the tree'.
(25) VṚKṢE VARṢĀ-BINDAVAḤ PATANTI 'The rain-drops fall on the tree'.

In each of these constructions VṚKṢA and PAT are related differently KĀRAKA-wise and features in which the two are compatible differ in each case. Their cooccurrence in any case is determined in terms of features shared by them whatever these may be.

From the above discussion it seems that any description of a KĀRAKA necessarily involves identification of (i) stem pairs and (ii) features shared by them. Let us see how PĀṆINI deals with this question.

PĀṆINI does describe in detail what stem pairs occur together in each KĀRAKA type. He stops at that. He does not describe in so many words why stems in a pair go together or what features characterize cooccurrence between them. The way he deals with the problem may be stated as follows.

From an analysis of linguistic data he distinguishes syntactic relations between nominal and verbal stems into two types, namely KĀRAKA and non-KĀRAKA. KĀRAKA relations are further sorted out in six types. Pairs in each type are assorted into one or more sets on the basis of their cooccurrence preferences or constraints. These sets for each KĀRAKA are described in 1424-55. His statements, though based on observation of facts in the linguistic data examined by him, limited in quantity, are being worded in generalized terms. These go beyond such data. Unique stems, on the other hand, are listed as such. His organization of stem pairs in the way he does is obviously designed to integrate it into his overall design of mechanism capable of producing various constructions in the language. A search for bases of their coocurrence is of little avail to him for this end. For this reason perhaps he does not go into the question of constraints on cooccurrence of nominal and verbal stem pairs as such. Features, linguistic or non-linguistic, linking pairs in a set described in structural statements can be easily worked out.

7. Analytical procedure to determine KĀRAKA types

Nominal inflections, as shown above, play crucial role in characterizing KĀRAKA relations. As a first step, one plausible and fruitful line of approach to determine nature and types of KĀRAKA relations, thus, may seem to be to consider simplex constructions with nominal stems ending in a particular VIBHAKTI type. Thus we come to gather an indefinite number of stem pairs marked uniquely with a formal feature, namely occurence of the same VIBHAKTI type. This cannot be brushed aside as accidental. Rather it strongly suggests of a common syntactic bond between all such pairs.

In the next step we may proceed to probe this assumption further by adding more data including complex structures. We may ask such questions as: Do these pairs share features, grammatical or otherwise, in other grammatical environments also? In other words, we propose to examine their overall grammatical behaviour at all levels of structure. If this group is found to hold together and displays consistency in its behaviour in different structural environments, we have legitimate and adequate grounds to recognize sameness of KĀRAKA relation between all such pairs.

We might even go a step further and claim that sharing of VIBHAKTI suffix alone by a group of pairs could be a reasonable basis for assuming identical KĀRAKA relation between them if such an assumption does not militate against any other well established KĀRAKA relation. There is nothing odd about it theoretically or pragmatically since VIBHAKTI suffix is a sine qua non of realization of KĀRAKA relation.

PĀṆINI'S analytical procedure for determining KĀRAKA relations, as outlined above, is simple and straightforward based on a close scrutiny of linguistic data. It hardly involves any abstruse and subtle assumptions of philosophical and metaphysical nature. It appears to be all common sense.

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For previous studies in KĀRAKA one may refer to George Cardona's : PĀṆINI : A survey of research, (1976). Mouton, The Hague. For the present paper I consulted the following. Reference to rules of the AṢṬĀDHYĀYĪ is from serial 1 below. The first numeral indicates the ADHYĀYA, second the PĀDA and the rest the SŪTRA. For instance 3319 means ADHYĀYA 3, PĀDA 3 and SŪTRA 19.

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Codeswitching in War and Peace

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Introduction

The language employed by L. N. Tolstoy in his monumental novel War and Peace is richly varied and intricate in design. His dialogue conveys with clarity and intense vividness the multiplicity of voices of the aristocracy, the military, the peasants and other groups, expressing a wide range of attitudes and emotions. Diverse characters, such as Pierre, Natasha , Andrej, Helene, Kutuzov, and many others are differentiated by their speech with a remarkable subtlety only attainable through the precise and intricate manipulation of language.

One of the most interesting and specialized aspects of Tolstoy's use of language in the novel is the prominence of the French language in dialogue and letters. The inclusion of French lends to the characterization of the Napoleonic era in Russia a realism that would be difficult to capture in any other manner. More importantly Tolstoy's use of French in his characters' speech serves as an effective stylistic device for conveying information relevant to the interpretation of the work itself. The purpose of this paper is to demonstrate how the consideration of such language material can be applied to a literary analysis and to suggest the extent and variety of such information indirectly conveyed in the language use pattern of the novel's characters.

Although there exists a substantial body of criticism on Tolstoy's style, relatively little of this writing is concerned directly with Tolstoy's use of language from a linguistic point of view. A great deal less of a systematic nature has been written about the use of French in War and Peace. A prominent view that has been put forward is that the alternation of French and Russian in the novel's character speech is used by Tolstoy primarily as a means of evoking a central meaning important to the thematic structure of the work (N.N. Naumova, 1959). According to this view, such variations in language usage serve to discriminate the positive characters of the novel, people of essentially high moral character who are closer in spirit to the Russian people (masses), from other characters, cut-off from the masses, whose behavior is seen as false and often deceitful.

The former category is said to include notably Pierre, Andrej, Natasha and the Rostov family in general, all of whom use French infrequently in the novel. The latter set includes such characters as Helene, Anatole, and Hippolyte Kuragin among others, who speak French extensively. Tolstoy's selection of Russian or French for those of his characters who are bilingual is also seen as a device for distinguishing occasions when they are expressing sincere, moral feelings and ideas from other moments when their speech reveals false or insincere behavior. For example, it is argued that Pierre expresses his feelings of love for Natasha in the Russian language because these feelings are sincerely felt. On the other hand, his feelings for Helene are expressed in French presumably because there is something less than honest or good about what he feels for her.

The use of the French language by characters is also interpreted as a means of expressing certain feelings and ideas of the author, specifically negative moral associations, connected with the French language. Naumov notes that in Pierre's dialogue with Captain Ramballe, Ramballe is able to express certain shameful and immoral thoughts with ease in French in a manner which tends to conceal their evil, while these same thoughts, had they been expressed in Russian, would have immediately revealed their true evil nature. In this same conversation it is noted that Pierre cannot tell Ramballe about his love for Natasha in the French language. Another example which is proposed deals with the use of French by Hyppolyte Kuragin, the weak minded elder son of Prince Vasilij and brother of Helene, at one of Anna Pavlovna's soirees. Hippolyte, who always speaks French in the novel, attempts to tell an anecdote in Russian. His joke makes him look quite foolish and perhaps even stupid. Naumov claims that Hippolyte's exclusinve use of French veils his inherent stupidity, which is instantly evident once he begins to express himself in Russian. What is argued from these examples is that Tolstoy uses the two languages to set up an invariable moral dichotomy between that which is false, unnatural and deceitful (bad) and that which is true, natural and sincere (good), with French signifying the negative qualities and Russian indicating the positive values.

There can be no doubt that the use of French and Russian adds a great deal of depth and verisimilitude to the novel. This becomes obvious by comparing the original text with any translation which fails to distinguish the use of the one language from the other, as both Maude and Garnet do by rendering all dialogue in English. It is also difficult to argue with the idea that the distribution of French and Russian usage has an important ideational significance for the novel. But all of this should be not be reduced to a simple ethical-didactic good/bad dichotomy at the expense of a more thorough analysis of bilingual usage in the novel. Overemphasis of such a dichotomy fails to catch many subtle and intricate nuances connected with the alternation of French and Russian in the novel.

Some aspects of Naumov's interpretation discussed above are contradicted by further evidence. For example, in the case of the passage about Hyppolite Kuragin, it should be noted that Hyppolite makes as big a fool out of himself on another occasion telling a joke in French. Other claims rely too heavily on the ethical opposition of "good" and "bad", where a more detailed analysis of the specific interractions involved and the social aspects of the context are necessary to understand the full significance of bilingual patterns. Investigations of the use of two or more languages by bilingual speakers in natural, everyday speech situations have shown that language switching of the sort found in War and Peace contains a variety of information on how one is to understand the words being spoken in relation to the particular context in which they are uttered.

The study of the significance of such switching is commonly understood to fall under the study of the pragmatic aspect of language, specifically the phenomenon of code switching. In this paper we begin with a general background section on the functions of linguistic code switching. This is followed by an analytical section which attempts to apply code switching to the analysis of dialogue from War and Peace, as a way to achieve greater precision in describing the interactions that occur in the work and in order to get at presuppositions and meanings relevant to literary interpretation.

The Code Switching Phenomenon

The use of two or more linguistic varieties in the same conversation or interaction is called codeswitching (Trudgill,1975). The varieties used may range from two genetically unrelated languages to two speech styles, of the same language, e.g. casual speech versus formal style. Other combinations, such as a switch from a standard variety to a regional dialect, are also included under this definition. The isolated usage of well established loan words or phrases, however, is not considered as part of the code switching phenomenon. Code switching which involves two genetically unrelated languages, such as the French/Russian code switching in War and Peace, is the simplest type to identify in a stretch of text (oral or written).

A speaker may switch codes for only one word or for longer stretches of speech. The other interactants in the verbal exchange may adjust their language behavior as a result of the code switching behavior or their behavior may remain unchanged. The most important characteristic of code switching is that this behavior is not performed in a random fashion. It is strictly rule governed, although the speaker may not always be able to consciously articulate these rules. The rules are primarily of a social nature in that the type of code switching that occurs may depend on such factors as the social roles of the interactants, the social situations in which the exchange is taking place, and a variety of other socially related factors.

Thus code switching is not simply ideosynchratic, whimsical behavior reflecting the speaker's individual choice, but conveys important information about the interrelationship(s) of the interractants and the social circumstances of the interaction (Timm 1975). The sociolinguistic rules that govern code switching are an integral part of the knowledge that the speaker must have in order to achieve his ends in interpersonal relations with other bilinguals. The listener must also have this knowledge in order to interpret the full meaning of the speaker and to make certain inferences about the speaker's intent in the specific context of the particular interaction taking place. Otherwise, effective communication cannot take place. Thus this aspect of sentence form -- the code in which an utterance is produced among bilinguals, can directly affect the interpretation of the utterance in the same way in which alterations in prosidy, rhythm and voice quality affect the interpretation of utterances used by monolinguals.

Ordinarily code switching is classified into two basic types. The classification is based upon various underlying social constraints present at the time of switching which in fact make the code switching possible and even probable. The two types of code switching are: situational code switching and metaphorical code switching (Fishman 1972).

Code switching of a situational type is tied to a concensus on the part of a speech community that a particular linguistic variety is most appropriate when the conversation or interaction involves a particular combination of topics, persons, locations, and purposes. That is, distinct varieties are designated as most appropriate for use in certain settings (home, work, etc.) or certain activities (public speeches, personal conversations, etc.) or certain categories of people (friends, strangers, public officials, etc.). In such situational code switching ordinarily only one code is used at a time. There is almost a one-to-one correspondence between language usage and the social context. Each variety has a distinct position in the local speech repertory. Norms or rules of language usage are stable and well established in the community and code selection can be viewed as conformance or as nonconformance to such rules. Classic examples of such speech communities where stuational code switching exists are the Hochdeutsch/Schweizerdeutsch variations in Swiss/German, the classical Arabic/colloquial Arabic variation in most Arab countries and the Katharevousa/Dhinotiki variation in Greek. In these situations typically the "high variety" (Hochdeutsch, Classical Arabic, etc.) are used for sermons, formal letters and lectures, and newspaper editorials, while the "low variety" is used in conversations with family and friends, radio programs, political and academic discussions and "folk" literature. Whenever a code is regularly associated with certain types of activities in such a manner it comes to connote these associations. Eventually its use even in absence of the other contextual clues can signal these activities.

The second type of codeswitching, metaphorical switching, also depends on socially defined usage rules as to the circumstances of code allocation, but the relationship of the language usage to the social context is much more complex (Gumperz 1976). In this type of codeswitching, the speaker utilizes and capitalizes on the understanding of the situational norms for code usage shared by himself and the listener(s) in order to communicate information about how the speaker's words are to be understood in this specific instance. The situational norm becomes thus a point of departure when relating a metaphorical message message (Gumperz and Hernandez, 1971).

This type of code switching is used to effect some specific set of inferences about the speaker's intent. Speakers rely on their sociolinguistically based knowledge about code usage to communicate (and decode) indirect conversational inferences. The monolingual also has devices available to him to accomplish similar types of ends, but the mechanisms available for manipulation obviously do no involve switching form one language to another. Even more commonly, he can alter the prosodic and rhythmic aspect of his speech. This greatly increases the range of options available to speakers to communicate indirect meanings in dialogues.

In general the grammatical distinction marking the two codes of the bilingual are a reflection of the contrasting cultural styles and attitudes with which these bilinguals deal in their daily encounters. The code associated with informal relations and in group activities is perceived as the "we code" while the code associated with more formal, out-group interactions is perceived as the "they-code" (Gumperz 1976). The associations influence the shifting of codes during interactions among bilinguals but are mediated by other aspects of the speech situation, such as discourse context and social presuppositions, so that they are not the sole factors involved in determining which code (or combination of codes) to use at a given time.

Code Switching in the Literary Dialogue of <u>War and Peace</u>

In this section we will consider instances of code switching occurring in opening lines of <u>War and Peace</u>, spoken by Anna Pavlovna Sherer as she greets her guest Prince Vasilij. The code switches will be considered in terms of both the type of function it illustrates and the particular effect that the speaker aims to achieve in communicating to the listener how the utterance is to be interpreted. Although French dominates in this pasasage, there are four code switches to Russian. For purposes of discussion theses are marked in the text below as C1, C2, C3, and C4 below:

> "Eh bien, mon prince, Gênes at Lucques ne sont plus des apananges, des /C1/ <u>pomest'ija</u>, de la famille Bounaparte. Non, je vous préviens, que si vous ne me dites pas, que nous avons la guerre, si vous vous permettez encore de paillier

> toutes les infamies, toutes les atrocitiés de cet Antichrist (ma parole, j'y crois) -- je ne vous connais plus, vous n'etes plus mon ami, vous n'êtes plus /C2/ moj vernyj rab, comme vous dites. /C3/ Nu, zdravstvujte. Je vois que je vous fais peur, /C4/ sadites' i rasskazyvajte."

Codeswitches C1 and C2 both involve approximate repetitions in Russian of what is said previously in French. In C1, the Russian word "pomest'ja" follows directly after the the French word "apanages" creating a parallelism (des apanage/ des pomest'ja). Codeswitch C2 also involves the use of a parallel construction, but in the form of two sentences, both of which begin with the words "vous n'etes plus.." In the parallel rephrasing of the statement, the sentence is completed by a Russian phrase rather than a French one. Generally the function of repetitions involving a codeswitch is to clarify, amplify or emphasize the message. This occurs in the two examples cited, but each produces slightly different effects and different sets of possible interpretations.

The words "apanages" and "pomest'ja" are very close in meaning, both referring to land grant estates owned by the higher ranking members of society. The switch to "pomest'ja" however, carries greater associations of the Russian lands owned and governed by the Russian people. The use of this word with all its local contextual connotations can possibly be seen as an indirect means used by the speaker (Anna Pavlovna) to emphasize in her statement the point that, although at the present time the "apanages" of Napoleon may be far off in Italy, who is to say that the land he may divide up in the future will not include Russian "pomest'ja." The use of the word "pomest'ja" serves to activate in the mind of the listener psychological associations and no doubt strong personal feelings connected with it, e.g. ones own estate, that of one's friends and relatives, a whole life style. The aim of the codeswitch is to dramatize for the listener the threat of the Russian land coming under the control of the Bonapartes, who in spite of the dependence of Russian royalty on French language and culture, are ideologically viewed as foreigners.

The second instance of codeswitching in the passage also involves repetition. Here the expression "mon ami" is followed by the Russian "moj vernyj rab" (my true slave). These two expressions, though similar in meaning, are much farther apart than are the words in the previous codeswitch. In French "ami" is the general word for a person with whom one shares a friendship. The Russian equivalent would of course be drug. The phrase "vernyj rab" represents a stronger expression of loyalty, devotion, and attention, far in excess of the more general word. There are certainly expressions in French which could come very close to conveying the connotations of the Russian expression. However, to understand the codeswitch in terms of its effect on the listener, it is necessary to examine briefly the probable usage of the Russsian expression in the society represented in the novel and in the specific relationship between the speaker (Anna

Pavlovna) and listener (Prince Andrej).

On the societal level, the phrase is no doubt a part of the entire set of verbal phrase, gestures, and other behavior that convey the code opf chivalry of the period with regard to relations betwwen men and women of royalty. We can say with some degrtee of certainly that this expression was used by either the listener or the speaker in prior encounters. Indeed, the subsequest French phrase "comme vous dites" would seem to indicate that both the French "mon ami": and the Russian "moj vernyj rab" are derived from corresponding assertions by Prince Andrej. Quotation is another type of function in metaphorical codeswitcvhing in conversation. The specific choice of Russian here, however, is significant for another reason. The precise word "rab" conveys a sensed of humility, honesty, and simpleheartedness (prostodušie) which is less likely to be felt with the same intensity by the speaker and listener if a French equivalent were chosen. Second, it emphasizes their bonds of mutual membership in Russian as opposed to French society, in which they are in fact the leaders of the Russian people. The use of the Russian phrase "moj vernyj rab" with all of these associations, thus serves to underscore the interpersonal consequences to the relationship between Anna Pavlova and the Prince, should he continue to defend the action of Napoleon ("si vous vous permettez....). Thus, here also, repetition of a similar form involving a codeswitch performs the conversational functiion of clarifying, amplifying, and emphasizing certain elements in the communication.

Codeswitches C3 and C4 are examples of another type of codeswitching occurring in conversation. They also function as a form of message qualification. However, whereas the preceding codeswitches performed a more localized function, C3 and C4 help to clarify the oraganization of all of the speaker's preceding utterances within this piece of discourse. The phrase "nu zdravstvujte, zdravstvujte" takes a form which from a societal standpoint might well serve as a more appropriate setting to the encounter that is taking place. It is precisely the sort of greeting that an invited guest like Prince Vasilij can, and most likely does, expect to receive from a close friend such as Anna Pavlovna. The subsequent Russian phrase "sadites' i rasskazyvajte" may be seen as a natural outgrowth and continuation of this greeting. The choice of Russian as the language in which the greeting, albeit belated is delivered, functions in the conversation as a marker of the degree of speaker involvement/distance of Anna Pavlovna from various parts of her entire statement. Codeswitches C3 and C4 taken together fall into the category of metaphorical codeswitching indicating personalization versus objectification.

The code contrast at this point (starting with "nu...") conveys the varying degrees of speaker involvement in different parts of the message. The Russian portions of this latter part of the text are Anna Pavlovna's expression of her personal feelings and

relationship with her addressees, while the preceding portions of the text, which are in French, (albeit with skillfully and subtlely inserted Russian words which emphasizxe and clarify particular points of her argument), is produced in the French language to emphasize the personal distance she feels from her arguments about Napoleon. In French she is expressing her opinions about a state of affairs in the world and these are the personal opinions of Prince Vasilij on this topic. But the Russian portions of the latter part of the text are an expression of her personal feelings and inclinations toward the Prince putting aside certain of his opinions on world affairs. The content of Anna Pavlovna's utterances produced in French are undoubtedly of no surprise to Prince Vasilij since they seem to have had at least one other discussion of the topic, as evidenced by Anna Pavlovna's use of "encore" in "si vous vous permettez encore de pallier....". This at least hints at the possibility that there has been a previous exchange of opinions between the two interactants on this issue before, and that she is particilarly distressed by new events relevant to the topic.

The fact that such verbal strategies of objectification and personalization have not been successfully conveyed is evidenced by the phrase "je vois que je vous fais peur" inserted between the Russian phrases of codeswitch C3 and C4. This is not a continuation of the arguments presented in French prior to C3, but rather her metacomment on the undesired effect of her entire preceding statement.

The final phrase of this opening text, "sadites' i rasskazyvajte" is a well formed and appropriate closing to this set of utterances and is a mechanism to turn the responsibility of speaking next to the listener, that is, this phrase allocates a conversation turn to Prince Vasilij, simultaneously terminating Anna Pavlovna's turn in speaking as well.

Broader Application of the Analysis of Codeswitching in Literary Communication

In the previous section we have presented a microanalysis of codeswitching in a short piece of literary text using the descriptive apparatus that has been developed to analyze codeswitching occurring in natural everyday conversations among bilinguals. The analysis indicates that in cases of metaphorical codeswitching, both in literary and natural conversations, such behavior is meaningful. It involves the use of language for purposes beyond the communication of simple factual information, and carries great potential in communicating indirect meanings which are essential to the interpretation of speakers' intent in a conversation. In the case of literary texts, speakers' intent is, of course, the intent the author envisions for the character. What is generated by the analysis of codeswitching, however, is not a single interpretation of the speakers' intent, but, rather, a set of preferred or possible interpretations, i.e. certain

chains of inferences which are favored over others. This is the case in both literary texts, and texts of natural conversations. Thus, the interpretation is never invariant, but the reliability of the addressee's judgments as to the intended interpretation is a result of familiarity with the way in which different types of codeswitching are to be understood in particular contexts. In everyday life, this is achieved through socialization of a speaker/listener into particular types of interpersonal relationships in a social community and knowledge of the rules of ethnically specific traditions.

In the case of literary communication, i.e. in a created text, the analysis and interpretation of communication as produced by the literary critic, must be achieved with knowledge gained from familiarity with the social and ethnic rules of the community represented in the text. In certain types of literary texts, this knowledge may be gained by the study of the period/community, e.g. by the study of the society of early 19th century Russia, the study of non-literary sources,such as letters, diaries, and through the careful analysis of the characterization of the period by the author -- the creator of the entire work within which the piece of text being analyzed is embedded. This latter point is important, since as in the case with War and Peace, the period depicted in the novel may not be a totally accurate and objective rendering of the Napoleonic period, but rather Tolstoy's personal vision and understanding of the period. In fairness to Tolstoy, we do know that he spent much time going over actual letters, documents, and memoirs from this period in preparing to write the novel. The task of capturing in all detail an era which is not ones own is, however, a monumental one. In works which are not historical, critics would have to rely on information provided by the author about the situations, events, and other aspects of the context of the conversation in order to make judgments on possible interpretations of codeswitches.

The analysis of the opening lines of War and Peace as spoken by Anna Pavlovna, presented in the previous section, is an illustration of how codeswitching is meaningful in a literary text and how a particular descriptive apparatus can be applied to instances of codeswitching in literary communication. As the thrust of this paper is primarily methodological, there is no attempt to relate this analysis to any other portions of the novel (See Note 1). It is our contention, however, that the analysis of codeswitching is useful in the overal interpretation of this literary text, and that, in fact, codeswitching of both the situational and metaphorical variety are manipulated by the author to convey important aspects of the interrelationships between characters, the contexts of the other details essential to the reading of the text. A thorough analysis of the codeswitching patterns in War and Peace are bound to be an important aid to a more complete understanding of both the novel and the mechanisms Tolstoy employs which distinguish him as a stylist.

The analysis of codeswitching has broader applications to literary analysis than only in the work discussed here. There are numerous instances of codeswitching in other Russian works depicting 19th century society written by Tolstoy and other writers. Works of authors from many other periods and of other linguistic cultures can also be analyzed using this methodology. In spite of the promising potential of such an approach for many such texts, it should be kept in mind that the analysis of bilingual usage in literature can serve only as one of many tools for interpretaion and not as an end in itself. The detailed analysis of codeswitching, using the particular framework and accompanying descriptive apparatus proposed here, offers information which must be integrated into a more complete analytical perspective.

NOTES

1. A further elaboration of this analytic approach applied to the dialogue of Tolstoy's War and Peace and to other works appears in D. Borker and O. Borker, The Sociostylistics of Literary Communication, ms.

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A Greek-Bulgarian Mischsprache in the Rhodope?*

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In an excursus in his Neugriechischen Studien II (1894: 90-92), Gustav Meyer discusses what he refers to as "Ein bulgarisch-griechischer Mischdialekt im Rhodopegebirge". The dialect in question was located in the Rhodope, in the area of Cépina, and was spoken by Pomaks, i.e. Moslem ethnic Bulgarians. This dialect had been described first by the Greek scholar Skordelis twenty years earlier,[1] who suggested that it was a form of Greek.[2] Meyer was rather of the opinion that it was a Bulgarian dialect, and it is clear that this is undoubtedly the proper view to take. Importantly, though, to judge from the title he gave to his excursus, it seems that Meyer felt that there was a further designation that was appropriate for this dialect; in particular, he seems to have believed that it could be classified as a "Mischdialekt", a "mixed dialect".

It is not my intention here to present new facts concerning this dialect, or even to delve at all deeply into the facts of the dialect as Meyer presents them. Indeed, Meyer's presentation is based on that of Skordelis, and Skordelis provided nothing more than a relatively brief word-list. There may indeed be more recent accounts available of this dialect (or ones just like it), but they need not be of any concern here.

Instead, I plan to consider carefully the nature of Meyer's claim that this is a "Mischdialekt", and in particular to examine just what this means in the light of the recent reopening of the question of "Mischsprachen"--"mixed languages"--by Sarah Thomason in several recent papers of hers[3] and by Thomason and Terrence Kaufman in their forthcoming book Language Contact, Creolization, and Genetic Linguistics[4].

I take as my starting point for this discussion (as do Thomason and Kaufman (p. 1)) a passage from Schuchardt (1884: 5) that, by opposing his own views to those of Max Müller, clearly lays out the controversy in the 19th century concerning "mixed languages", i.e. languages whose genetic origins are such that they are the offspring of more than one "parent" language, instead of showing the more familiar single parent origin:

> Mit mehr Recht als MAX MÜLLER gesagt hat: "Es gibt keine Mischsprache", werden wir sagen können: "Es gibt keine völlig ungemischte Sprache".

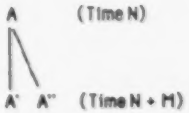
Müller's actual statement on the subject, as taken from his Lectures on the Science of Language (Müller 1878[2]), is worth citing, though, for it brings out an important distinction in the controversy and points to the need for some clarification. It runs as follows: 'We [have] had to lay down two axioms ... the first declares grammar to be the most essential element, and therefore the ground of classification in all languages ... the second denies the possibility of a mixed language' (p. 82). It is this second axiom that Schuchardt has apparently picked up on in the passage cited above. Müller goes on, however, to say that 'there is hardly a language which in one sense may not be called a mixed language. No nation or tribe was ever so completely isolated as not to admit the

importation of a certain number of foreign words.' Thus, what Müller is claiming in denying the existence of "mixed languages" is that there are no languages with "mixed grammar". He does, though, seem to be allowing for language mixture in a trivial sense, i.e. that which arises through lexical borrowing. However, there is a certain vagueness in talking about grammar in this context: for example, would borrowed morphology or borrowed syntactic patterns constitute "mixed grammar"? To a certain extent, then, what is at issue is the definition of "language mixture". It is to this particular facet of the question that Thomason and Kaufman make a contribution, by providing a clear characterization of "mixed language", thereby making it a technical term.

Their technical characterization of "mixed language" has to be understood in terms of the significance of the controversy regarding language mixture, summed up so neatly in the above quotation from Schuchardt. The importance of the possible existance of such a language type stemmed from the state of linguistic research in the 19th century. This period was witness to truly spectacular achievements in historical linguistics, and these advancements were made possible largely through the development of the methodology now known as the Comparative Method. Mixed languages, if they existed, posed a threat to the findings of historical linguists and to the methodology they used because mixed languages, by their very nature, run counter to the basic assumptions that allow the comparative method to work.

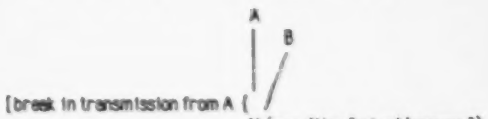
In particular, successful application of the comparative method depends on an assumption of direct lineal descent on the part of two or more languages from some common source, as indicated in (1), where N is some arbitrary point in time taken as the starting point for the investigation, N + M is some arbitrary point in time later than N, and A' and A" are changed forms of A, and thus are offspring of A:

(1) Direct Lineal Descent



If, on the other hand, the descent through time from the language stage A to later forms of A involves a significant influence from another language in a particular set of social circumstances, there can be a break in the direct lineal transmission of A to subsequent sets of speakers; if such an "imperfect transmission" of A occurs--to use the descriptive label of Thomason and Kaufman--as in (2), then the resulting language in a sense has two parents, i.e. is a mixed language:

(2)



In such a case, if a mixed language results, then, Thomason and Kaufman argue, "the label 'genetic

relationship' does not properly apply" (p. 15)--A' is not the direct lineal descendent of A but rather is "agenetic" as far as linguistic family relationships are concerned. Accordingly, in such a situation, the Comparative Method could not draw on or be applied to the outcome of these developments.

Normally in language contact, the extent of the influence of one language over another is neither so great nor so drastic nor so intense over a relatively short period of time, e.g. causing language shift within one generation, that it leads to a true break in transmission. It is also not the case that the necessary social factors are generally present that are conducive to such a break--e.g. the failure of the shifting group to be fully "integrated into the group which provided it with a new language" (Thomason & Kaufman, p. 15). However, the conditions necessary for such drastic breaks in direct lineal transmission of a language have demonstrably occurred in the past, thereby creating truly "mixed languages", languages that have arisen by a peculiar set of social circumstances possible (but not necessary) when two (or more) speech communities come into contact with one another.

Examples of such mixed languages include pidgins and creoles--Tok Pisin, the emerging creolized national language of New Guinea is one such case--but also certain nonpidgin/noncreole[5] languages such as Ma'a,[6] an African language with a Cushitic lexical base but Bantu grammatical structure, or Michif,[7] the language spoken by many residents of the Turtle Mountain Chippewa reservation in North Dakota that generally has French nouns and adjectives--together with their structural patterns--but Plains Cree verbs and verbal syntax. Thus it seems that mixed languages do exist; they are perhaps not to be found to the extent that Schuchardt believed, but neither are they the impossibility that Müller believed them to be. What makes them rare is the fact that the social circumstances that can lead to these truly mixed languages do not frequently arise in language contact situations.

With this background concerning mixed languages, and armed now with a definition that allows the notion "mixed language" to be treated as a technical term, the Rhodope dialect reported on by Meyer can be examined. The facts that Meyer gives for this dialect are a list of 23 verbs built up of a Greek stem--specifically the aoristic (i.e. perfective aspect) stem--with a productive Bulgarian derivational suffix, <u>-ova-</u>; the forms in question include those in (3):

(3) argaovam[8] 'I work' (cf. Greek ἐργάζομαι [ergázome], aorist stem ἐργασ- [ergas-])
arnisovam 'I deny' (cf. Greek ἀρνοῦμαι [arnúme], aorist stem ἀρνησ- [arnis-])
diakonisovam 'I serve' (cf. Greek διακονῶ [ðjakonó], aorist stem διακονησ- [ðjakonis-])
zalisovam 'I am dizzy' (cf. Greek ζαλίζομαι [zalízome], aorist stem ζαλισ- [zalis-])
prokopsovam 'I succeed' (cf. Greek προκόπτω [prokópto], aorist stem προκοψ- [prokops-])
xarisovam 'I give' (cf. Greek χαρίζω [xarízo], aorist stem χαρισ- [xaris-])
xonepsovam 'I digest' (cf. Greek χωνεύω [xonévo], aorist stem χωνευ- [xonevs-]).

Meyer also lists several nouns as well that are from Greek, as in (4):

(4) gunía 'corner' (cf. Greek γωνία [γonía])
drum 'road' (cf. Greek δρόμος [ðrómos])
zune 'bell' (cf. Greek ζώνη [zóni])
kromit 'onion' (cf. Greek κρομμύδι [kromíði]).

It turns out, as Meyer observes, that the nouns of Greek origin in this dialect are ones that are also found elsewhere in Bulgarian; that is to say, their existence in this speech community need not be directly due to Greek influence but instead--if this is a Bulgarian dialect, as Meyer suggests--they could simply be in this dialect as the result of direct lineal descent from the source of Bulgarian dialects.

This last observation is important in the context in which this discussion began, namely in the context of a concern for the existence of "mixed language forms", for it suggests that this Rhodope speech community is not truly a "Mischdialekt", e.g. Greek with Bulgarian grammar, but rather is a dialect of Bulgarian. Furthermore, the evidence presented in (3) is consistent with this view, for these facts show only the effects of a very common occurrence when loan words enter a language--the words are adapted to the borrowing language's morphological patterns. In this case, Greek aoristic verb stems were made over in the borrowing language, Bulgarian, with productive Bulgarian derivational verbal morphology, i.e. the suffix -ovn-. Since the source of this suffix is Bulgarian, it would presuppose--or perhaps demonstrate--that the dialect in question is a Bulgarian dialect, one that happens to have an overlay of some Greek lexical input,[9] and not a "mixed dialect".

Morphological reshaping of loans is such a common development in instances of language contact that it probably does not need exemplification, but in (5) some examples are given that are especially relevant to the matter at hand of Greek verbal forms being borrowed into the Čépina dialect of Bulgarian and morphologically remade. In these examples in (5), Turkish simple past tense verbal stems have been borrowed into Greek and remade with a native Greek verbal derivational suffix, -ίζω:

(5) γλεντ-ίζω [γlendízo] 'I celebrate' (cf. Turkish eğlen-mek (INFINITIVE), simple past stem eğlendi-)
κοβουρντ-ίζω [kovurdízo] 'I roast' (cf. Turkish kavur-mak (INFINITIVE), simple past stem kavurdı-)
μπαιλντ-ίζω [baildízo] 'I faint' (cf. Turkish bayıl-mak (INFINITIVE), simple past stem bayıldı-)
μπογιαντ-ίζω [bojadízo] 'I paint' (cf. Turkish boya-mak (INFINITIVE), simple past stem boyadı-)
ντεβρεντ-ίζω [devrendízo] 'I strengthen' (cf. Turkish devren-mak (INFINITIVE), simple past stem devrendi-)
ντεγιαντ-ίζω [dejandízo] 'I beer' (cf. Turkish deyan-mak (INFINITIVE), simple past stem deyandı-).

While it is not entirely clear why past stems, in the case of the Turkish loans, or aoristic stems--one of whose functions is in the formation of completive aspect past tenses--in the case of Greek loans, should have been the basis for the borrowings and reshapings noted here,[10] other such instances are apparently to be found all over the Balkans.[11] At the very least, though, the examples in (5) show that what happened in the Čépina dialect of Bulgarian is not at all unusual.

This Rhodope dialect that Meyer labelled as a "Mischdialekt", then, probably is not mixed, at least not in the now technical sense of "mixed language" developed by Thomason and Kaufman, nor even in Müller's vague sense of showing "mixed grammar". It is a "mixed dialect" only in the most trivial sense, i.e. only in that there are some nonnative, i.e. non-Bulgarian and specifically Greek--elements in the dialect, most evidently in the form of the stems of a class of verbs. Thus it is mixed just in the one way in which Müller himself sanctioned the notion of language mixture, i.e. via lexical borrowings. It is not, however, a speech form that has arisen under the special conditions that seem necessary to bring on a wholesale shift by a speech community from one language to another in a short period of time with a consequent break in the normal lineal transmission of a language through generations and peer groups. While it is not entirely clear exactly what Meyer had in mind when he referred to this dialect as a "Mischdialekt"--he does not elaborate on this designation at all in his brief discussion--and he may simply have meant that it is mixed in the trivial sense and not in any more significant sense, the discussion here can be taken as a step in the direction of clarifying our understanding of the nature of this dialect.

Given then that this Čépina dialect shows the effects of language contact, it is of course interesting and important to speculate on the nature of the Greek-Bulgarian contact in Čépina that led to the intimate borrowings evident in (3) (and possibly (4)), whereby Greek words were borrowed and incorporated in Bulgarian, replacing already-existing Bulgarian words. It is the case, however, that intimate borrowing represents the characteristic type of contact situation found throughout the Balkans, so that on this count also, the Čépina situation is not at all unusual.

Although this Rhodope dialect has not proven to be a mixed dialect in any interesting sense, the fact that true noncreole mixed languages do exist, as shown by Michif and Ma'a, means that the possibility must always be taken seriously that a given language contact situation under investigation might be such as to produce a mixed language. Moreover, given the relative rarity of mixed languages, it is essential to investigate (within reason) every reported instance of mixed language forms in order to see if more can be uncovered. This investigation of the limited data from Meyer can be taken in that vein. In the case at hand, the investigation did not lead to the uncovering of another mixed language, but it did lead to a clarification of Meyer's report on this Bulgarian dialect. Furthermore, it is not inconceivable that somewhere in the Balkan peninsula, now that it is clear what to look for and how to look for it, evidence of a truly mixed language might turn up.

Notes

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for useful and insightful comments, though I have not necessarily followed up on all their observations and suggestions in this version.

1. Meyer cites the work as Skordelis 1874, though the copy that I have of the piece (a xerox only, without the title page of the journal, however) indicates instead a date of 1875.

2. The context of Skordelis' observations on Greek in the Rhodope is relevant to understanding his suggestion. Skordelis was responding to doubts raised by other European scholars about the ethnic origins of the modern Greeks (in particular whether they were "true" descendants of the ancient Greeks or instead were perhaps Slavs or even of some other origin). He presented this Greek vocabulary to demonstrate that this part of the Rhodope had been at some point in the past primarily a Greek-speaking, and thus Greek, area. See Joseph 1985 and references therein for some discussion of other instances of Greek scholarship responding to similar claims.

3. For example, Thomason 1980, 1983, 1984.

4. I owe a great debt of thanks to the authors for providing me with a prepublication copy of their manuscript and for permission to quote from that version.

5. These languages are not to be considered creoles because they do not show any significant degree of morphological and morphosyntactic simplification in comparison with their source languages, whereas true creoles generally do.

6. See Thomason 1983 for detailed discussion of Ma'a with extensive bibliography.

7. The major sources on Michif are Crawford 1976, Evans 1982, Rhodes 1977, and Weaver 1982; see also the discussion in Thomason 1984 and Thomason & Kaufman (Chapter 9).

8. The initial a- here, as opposed to the ε- in the Greek form cited here, probably reflects a Greek dialectal development in the dialect that provided the input to the Bulgarian dialect in question.

9. This evidence would also, of course, argue against Skordelis' identification of this dialect as a form of Greek.

10. It may be, for example, that the aoristic and past tense forms are of greater frequency than nonaoristic or nonpast forms, or that they are the unmarked members of verbal paradigms. I know from personal experience that I mastered the Greek aoristic past tense long before I ventured into the realm of the imperfective past forms. Still, if frequency or markedness were responsible, one might expect that the generalization of one stem as opposed to another might be lexically governed, varying with the semantics of the borrowed word itself. Thus some further explanation for this Balkan (or actually pan-Balkan--see footnote 11) parallel may still be needed.

11. It is worth noting that already in the last century Miklosich had observed that aorist stems were the basis for verbal loans in the Balkans. I am indebted to Eric Hamp for bringing this fact to my attention.

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Feminism in Morphology

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1. Introduction

In 1970/71, when I had the privilege of serving at the Department of Linguistics of OSU under the chairpersonship of Ilse Lehiste, she and other members of the department were already actively interested in the feminist movement. As successful and less successful coinages (chairperson for chairman vs. woperson for woman) demonstrate, an area of great theoretical activity in the department has been affected by feminist innovations, i.e. morphology.

In this paper, I will bring together both interests and will try to show how feminist morphological innovations can be judged in the light of theoretical morphology. My purpose is neither to promote nor to reject proposed innovations, but to evaluate their relative degree of actual or potential acceptability as far as morphological parameters are concerned. The data analyzed will be German ones, not only for reasons of accessibility to me but also because in German both the opposition of masculine and feminine gender (e.g. the definite singular articles der (Nom. masc.) vs. fem. die) and feminine gender derivation (e.g. Löwe 'lion' → Löw-in 'lion-ess') play a greater role than in English.

The theoretical model espoused is that of Natural Morphology (cf. Dressler 1985a,b,c; Mayerthaler 1981; Wurzel 1984; Dressler et al., in press; Studia gramatyczne 7/1985, on Natural Approaches to Morphology), a theory of universal morphology where several morphological parameters are studied: e.g., the parameter of morphotactic transparency refers to the respective degree of obstruction to morphological processing (e.g. morphonological rules render the identification of conclude in conclusion more difficult than phonological resyllabification the identification of exist in exist+ence. Or the parameter of morphosemantic transparency refers to the degree of semantic compositionality of a morphologically complex word.

What should be differentiated from morphosemantics and established as a separate subdiscipline, is morphopragmatics (cf. Dressler & Merlini, in press). This term refers to the area of relations between morphology and its pragmatic setting (language users and language usage within the speech situation with its presuppositions). In semiotic terms and in regard to our topic, the specific (pragmatic) attitude of an interpreter towards the signatum of a sign concerns morphopragmatics in regard to specific motivations of complex words by specific (groups of) interpreters.

2. Feminism and Morphopragmatics

Irrespective of historic origin (cf. Wienold 1967; Ibrahim 1973) correlations between grammatical gender and sexus (e.g. masculine - male, feminine - female, neuter - inanimate) are only partially effective in German grammar and lexicon. However, gender has a potential of being semantically interpreted (Wienold 1974: 315; cf. Wienold 1982 for psychoanalytic consequences

of this potential). And this was the case when feminist linguists looked for linguistic symptoms of male supremacy (cf. Ritchie Key 1975: 68ff; Trömel-Plötz 1978; Hellinger 1985).

They were not only concerned at cases of sexist unequality similar to the English pair master - mistress, but also by gaps in the lexicon such as *die Bau-frau alongside der Bau-herr 'building contractor' (in view of Frau 'Mrs., woman, madam', Herr 'Mr., Gentleman, master, Sir') and at the generally unmarked status of masculine gender (Spender 1980: 19ff; Hellinger 1985; Pusch 1985; Kalverkämper 1979). E.g. in sex-related gender derivation German feminine nouns are usually derived from masculine forms rather than masculines from feminines (cf. Trömel-Plötz 1978: 56; Kalverkämper 1979: 59; Plank 1981: 96ff, 116ff), e.g. in animal names the type der Löwe 'lion' → die Löw-in 'lioness' is usual, the type die Gans 'goose' → der Gäns-erich 'gander' is exceptional. And whereas feminine -in motion is easy from nouns denoting men, such as der Herr- 'lord' → die Herr-in, der Jäger 'hunter' → die Jäger-in, Prior 'prior' → Prior-in, the only example of the opposite direction is der Hex-erich 'sorcerer' (a variant of deverbal der Hex-er from hex-en 'to practice sorcery') derived from die Hexe 'witch'. Cf. also the isolated Witw-er 'widower' from Witwe 'widow'.

Moreover among many pairs der X - die X-in the feminine form may only refer to females, whereas the masculine form either refers to males or is sex-neutral (generic, cf. Kalverkämper 1979; Glück 1979), e.g. der Kunde 'client' may contrast with die Kund-in, but may also be used for both sexes especially in the plural die Kund-en. In inflection, gender is clearly differentiated in the singular (always with the article; feminine nouns never have case suffixation, masculines and neuters may have) but neutralized in the plural of the articles and generally in the distribution of plural suffixes. Thus it is only the absence of the feminine derivational suffix -in which allows the inference that die Kunde-n refers to males (vs. fem. die Kund-inn-en). But see § 5b.

Now male and female addressees may feel more and less addressed by "ambiguous" (i.e. male and generic) nouns (cf. Kramer 1978: 95; on the other connotations see Zubin and Köpcke 1984), but clearly, seen from a morphopragmatic point of view, feminist interpreters are much more likely to interprete such nouns as excluding females because the fem. suffix -in is lacking (cf. Trömel-Plötz 1979: 126; Guentherodt 1979: 126; Hoffmann 1979: 60; Pusch 1985; Hellinger 1985: 30). In other words, the generic vs. male vs. female interpretation of generic nouns depends on the pragmatics of both linguistic context (e.g. inclusion into anaphoric chains of coreference, cf. Kalverkämper 1979: 64ff) and context of situation, including the interpreter's sex and attitude towards feminism.

Notice also that generic nouns such as der Mensch 'human being', die Person 'person' which have no 'heterosexual' counterpart (die *Mensch-in, der *Persön-erich) may have a sexist interpretation due to the grammatical article, e.g. der Mensch may be considered as referring rather to men, die Person rather to women, whereas nothing points to a specific sex in their plurals.

3. Feminist strategies

In antisexist language policy several strategies have been proposed for changing either official or unofficial language use or merely for arousing attention to the issues involved in order to allow equal and unambiguous reference to the intended sex (including sex neutrality) (cf. Kodak et al. 1986; Hellinger 1985; Pusch 1984, 1985). They are as far as morphology is concerned:

1) "Splitting" instead of the masculine and/or generic term: e.g. der Student 'student' → der Student und die Student-in, der/die Student/in. This strategy has often been attacked as uneconomical (e.g. Kalverkämper 1979: 63).

2) Replacement of the more sex-relatable singular by the less sex-relatable plural (cf. §2): e.g. der Student → die Student-en (instead of splitting: die Student/inn/en).

3) Replacement by truly sex-neutral terms: e.g. die Studier-end-en 'the studying persons' (but notice the singular contrast between der Studier-ende/ein Studier-ende-r and die/ein-e Studier-ende).

4) Replacement by collective or functional terms: e.g. der Student → die Student-en-schaft 'the studentship'.

5) Coining of new terms (in addition to the other strategies).

The adequacy of these strategies and the acceptability of their results depends on many factors (cf. Schräpel 1985; more general Allony-Fainberg 1977) of which I will discuss a few morphological ones.

4. Sexist motivation and remotivation

Endeavours towards reinforcing the use of die Kund-in for female clients (§2, 3.1) instead of generic der Kunde presupposes the morphopragmatic identification of male reference of der Kunde. However the probability of male reference of ambiguous terms depends in several factors.

a) The first factor is the morphological makeup of the word. In der Kunde, der Landwirt 'farmer' only the article points to masculine gender, but neither the root Kund-,-wirt nor the stem-suffix -e in Kunde-n.And the plurals die Kunde, die Landwirt-e could belong to a feminine declension class as well, in contrast to oblique singular case forms, e.g. gen. des Kunde-n, Landwirt-s.

b) In contrast, suffixations that have an animate meaning are much more sex-related: -in (and -essin), French derived -ess, -euse, -ine, -ière always refer to females, -erich to males (§2). Nouns in -er (variants -ler, -ner, cf. -iker, -ianer) -ling, -ian, -ikus, -(at)eur, -ar, -(at)or, -ist, -ent, -ant, -är are either male-related or generic. However, a suffixed word such as Bau-er 'peasant' has a much higher probability of referring to males than its non-suffixed synonym Landwirt. Thus, ceteris paribus, it is easier to portray a suffixed word as male-related than a suffixless one and therefore to call for a specific female-related term. And in fact die Bäuer-in is much more used than die Land-wirtin instead of including females into Bau-er and Landwirt. Of course knowledge of sex-relatedness of foreign suffixes seems to correlate with knowledge of learned vocabulary (of foreign origin) in general.

c) Morphosemantically transparent (compositional) compounds (cf. §1) behave like their heads. E.g. der Land-arzt 'country doctor' and die Landärzt-in have exactly the same relation as der Arzt 'doctor' and die Ärzt-in.
d) This is not the case with morphosemantically opaque compounds, whose potential sex-relatedness may be even lower than with simplex words in idioms. Let us analyze Mann 'man' and Herr 'master', gentleman, lord': Idioms such as Herr der Lage sein 'to be master of the situation', Herr im Hause sein 'to wear the trousers (lit. 'to be master in the house)', seinen Mann stellen 'to hold own's own' may be used by women referring to a woman (of course the masc. possessive sein-en 'his' must be changed to fem. ihr-en 'her') although many speakers (not only feminists) may not find it very adequate or elegant. Also the com. Alle Mann an Deck! 'all hands on deck!' may refer to women as well. The problem of which idioms allow this, and why the plurals Herr-en and Männ-er (for compounds cf. Samoilowa 1970) are always male-related, need not concern us here.

Similarly in all opaque compounds (i.e. non-compositional morphological constructions) the male-relatedness of -mann (cf. Samoilowa 1970) and -herr is greatly reduced (in a synchronic sense). Some examples are Land-s-mann 'compatriot' (vs. Land-mann 'peasant'), Vorder-mann 'man ahead' (vs. 'front-rank man'), Stroh-mann 'man of straw', Dunkel-mann 'obscurantist' (translation of humanist Latin vir obscurus), Ob-mann 'head-man, chair-man' (lit. 'over man'), Bieder-mann 'man of honour/worth' (lit. 'loyal man'), Haupt-mann 'captain' (lit. 'head man') either as a military title or in doubly opaque compounds such as Landes-haupt-mann 'chief executive/governor/president of a federal province', Burg-haupt-mann, Stadt-haupt-mann, Schloß-haupt-mann, Deich-haupt-mann; the title der Groß-herr (lit. 'great master'), Brot-herr 'employer' (lit. 'bread master'). Reinforcement of sex-relatedness in such compounds is an instance of morphopragmatic remotivation.
e) This last group also shows the second factor, the lexical factor of idiosyncratic global development.
f) Sex-relatedness of a base may be reduced by derivation and compounding (which makes it a non-head). Examples with Mann and Herr are Mann-schaft 'team' (lit. 'man-ship'); Herr-schaft 'mastery, command' (lit. 'master-ship'), mann-s-hoch 'tall as a man'. Establishing sex-relatedness (cf. Hoffmann 1979: 115f) is an instance of higher morphopragmatic remotivation than in d). For there (e.g. in Brot-herr, Ob-mann), it is the head whose sex-relatedness has to be reinforced, whereas here it is the non-head; and the head is known to be more important for the semantic categorization of a complex word than the non-head (cf. § 5 h).
g) An extreme case of remotivation is back-formation, of which I could not find any interesting instances. E.g. die Näh-er-in 'needle woman' and die Zu-geh-er-in 'charwoman' (lit. 'to-go-er-ess') are females without male counterparts (for social reasons). The masculine potential bases der ?Näh-er, ?Zu-geh-er do not exist, but are potential words (and false intermediate steps in derivation from näh-en 'to sew' and zu-geh-en 'to go up to'),

although the comparative näh-er 'nearer' might exercise some homophony blockage.

5. Feminist neologisms

a) Whenever a female-related counterpart of a male-related or ambiguous term (§ 2,3) is lacking, this gap may be filled by a neologism (cf. § 3). Clearly productive word formation rules must be used. Also, conditions on potential bases (e.g. on concatenating native bases with foreign suffixes and vice versa; -in may not be combined with -ling, cf. Wellmann 1975: 109) and connotative restrictions must be respected.

E.g. compounds (or 'suffixoid formations') with -liese, -suse, -trine must not be proposed because they are all pejorative, as in die Heul-suse/trine 'cry-baby'. Similarly masc. -erich is difficult to use in suffixation since it is either pejorative (e.g. Wüt-erich 'blood thirsty villain') or only applicable to bases designating animals (e.g. Gäns-erich 'gander').

b) Exocentric (possessive) compounds seem to resist feminine gender derivation. E.g. der Trotz-kopf 'pig-headed person' (lit. 'defiance head') or der Dick-häut-er 'pachyderm' (lit. 'thick-skinn-er') do not lend themselves to derivations such as die *Trotz-köpf-in, *Trotz-kopf-frau, *Dick-häut-er-in, *Dick-häut-in, *Dick-häut-er-frau (unless in the transparent meaning 'spouse' of a pachydermic male'). The reason seems to be that such compounds are truly and only generic. Therefore Trotzkopf is the name of the heroine of a successful series of novels about and for girls who identify with her. Notice also that die Rot-haut 'redskin' has no female denotations or connotations despite its feminine gender, since the semantic base where sex-relatedness could apply to is, as it were, outsides. Thus article inflection alone is not capable of attributing sex connotations. Therefore generics such as der Kunde (§ 2) must be truly ambiguous, i.e. their male-related reading (although maybe often of a latent status) is not a mere invention of feminists.

c) Otherwise -in suffixes and compounds with -frau 'woman' added to words or replacing -mann or -herr in compounds can be freely used (cf. Hellinger 1985; Pusch 1984: 26ff, 35ff; Guentherodt 1979) with the following caveats:

d) The addition of -frau must compete with a series of compounds where X-frau designates the spouse of X in contrast to X-in 'female X', e.g. Lehr-er-in 'female teacher' vs. Lehr-er-frau 'spouse of a teacher' (Plank 1981: 116ff). However this does not entail violating a word formation rule, because such series have only the status of lexical fields (more in Pounder, in press).

e) If -frau replaces -mann or -herr in an opaque complex word, resistence of the speech-community to such neologisms is likely to be greater than in a transparent word... For not only the neologism must be accepted but also the presupposed sexist remotivation. Therefore Frau-schaft as a counterpart to Mann-schaft or Herr-schaft (§ 4f) seems to be unacceptable.

If our gradation of remotivation in §4c-f is correct, then replacing -mann, -herr with -frau in opaque compounds (§ 4d) must be more acceptable, but still less than in transparent compounds. And in fact ?Stroh-frau, ?Dunkel-frau, ?Brot-frau seem to be hardly conceivable as counterparts to Stroh-mann, Dunkel-mann, Brot-herr. Ob-frau alongside Ob-mann is used but resisted;

but Ob-mann itself is such an awkward officialese title that maybe Ob-frau does not sound much funnier.

The proposed Landes-haupt-frau as the designation of an (as yet non-existent) female Landes-haupt-mann is unfortunate for two reasons: 1) because Landeshauptmann is doubly opaque (§ 4d). 2) because Landeshaupt-frau may be felt to contain the noun Haupt-frau 'main spouse' (of a polygamist).

f) One way out might be thought to be -in suffixation. In fact there exists Land-s-männ-in 'female compatriot' in contrast to Land-frau 'peasant woman' as a counterpart to Land-mann. Vorder-männ-in 'female ahead' is usable but unlikely to be accepted. Other conceivable forms in -männ-in seem to be still less aceptable. The proposed transparent form Amt(s)--männ-in 'female magistrate' has been rejected (Guentherodt 1979: 128; Hoffmann 1979: 109). Landes-herr-in 'female sovereign' exists as counterpart to Landes-herr 'sovereign', but only because Herr-in (where social status is more important than sex-reference) exists and Landes-herr 'lord of the country' is transparent. The same holds for Ober-herr-in ← Ober-herr 'supreme lord', Grund-herr-in ← Grund-herr 'lord of the manor'. Since -in forms of opaque Groß-herr, Brot-herr seem rather awkward, -in suffixation seems not to be a viable alternative. Of course the attested forms Mitglied-er-inn-en 'female members', Schluß-licht-er-inn-en 'female tail-lights' (metaphorical) are ungrammatical nonce-formations since they have two plural suffixes interrupted by the derivational suffix -in, a double violation of German morphology.

g) Thus extramorphological strategies must be employed, such as replacing Landes-hauptmann (e) with e.g. Landes-präsident → Landes-präsident-in or speaking of der weibliche Landes-hauptmann 'the female L.'.

h) We have seen (§ 4f) that the head position is semantically more critical than the non-head position. Thus feminizing the head is more important and noticeable than feminizing the non-head. E.g. at the end of the Austrian national elections of 1986 certain politicians thanked their Wähl-er-inn-en und Wähl-er 'female and male voters' (or inverse order), but all of them spoke of the term Wähler-gruppen 'voter groups', nobody in addition of Wähler-innen-gruppen. Therefore the splitting of Dienstnehmer 'employee(s)' into Dienstnehmer/innen (cf. § 3) is easier to promote than the replacement of Dienstnehmer-recht(e) 'employee(s) rights' with Dienstnehmer/innen-recht(e).

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Paragraph Perception by Seven Groups of Readers

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1. Introduction

When teachers of basic writing read their students' writing, they are often puzzled by the paragraphing they see. Some basic [remedial] writers never indent; some indent almost every sentence while others follow more traditional paragraphing strategies. The indentations themselves may seem insignificant, simply a part of the code of written language not yet fully mastered. But the incoherent and underdeveloped essays in which the irregular paragraphing often appears is of concern to everyone interested in the development of basic writers and in the general intricacies of literacy.

2. Background

Whether or not paragraphing is an issue worthy of investigation depends on assumptions about the validity of written language as an object of study. Bloomfield, in his efforts to direct linguistic study to oral language, demoted the value of written language: 'Writing is not language, but merely a way of recording language by means of visible marks. . . . We have to use great care in interpreting the written symbols into terms of actual speech; often we fail in this, and always we should prefer to have the audible word' (1933: 21). Critics of the study of paragraphs have cited an absence of paragraphing in oral language to support their position, believing that paragraphs were characteristic only of written language. After all, the term paragraph itself refers to a mark that appears 'beside writing' and is not related etymologically to speech. Those disinterested in paragraphing can cite rhetoricians such as Corbett to support their position, for according to Corbett, 'Paragraphing, like punctuation, is a feature only of the written language' (1971: 477).

Other researchers, such as Pike, believed that there were grammatical units larger than the sentence: 'A bias of mine--not shared by many linguists--is the conviction that beyond the sentence lie grammatical structures available to linguistic analysis, describable by technical procedures, and usable by the author for the generation of the literary works through which he reports to us his observations' (1964: 129).

Testing Pike's conviction, Koen, Becker, and Young (1969) conducted a study designed to determine the psychological reality of the paragraph. Their subjects were asked to mark sentences as paragraph openers in several continuously typed versions of a text. Their findings supported the hypothesis that paragraphs could be identified in written language. But no research had yet been conducted to determine whether there were paragraphs in speech.

In spite of his statement that paragraphing pertains only to written language, Corbett does acknowledge the contribution that paragraphing makes to readability of printed prose. He also suggests a basis of paragraphing in

oral language, for he predicts that a student, when asked to read 'a passage of prose with no punctuation, capitalization, or paragraphing, . . . might eventually be able to make sense of [the] passage,' especially 'if he reads it aloud, because the voice will add another grammatical element, intonation, which is the vocal equivalent of the graphic marks of punctuation' (1971: 448). The grammatical element that Corbett identifies as intonation and that is equivalent to paragraphing has now been shown to exist in speech.

As the domain of phonetic studies has increased from the segmental to the suprasegmental, from isolated sounds and words to sentences and connected discourse (with the development of the equipment necessary to conduct such research), knowledge of the phonetic characteristics of discourse has been revealed in Lehiste's seminal studies that show the existence of paragraphs, or their equivalent, in oral discourse. Lehiste has conducted a series of investigations of connected discourse that are summarized in her article, 'Some phonetic characteristics of discourse' (1982). She reports that 'Three phonetic factors appear to interact in providing paragraph boundary cues: length of pause, presence of laryngealization, and preboundary lengthening' (1982: 125). She concludes that 'the research . . . demonstrates the perceptual reality of phonological units consisting of more than a single sentence' (1982: 126) and that 'listeners agree among themselves about the presence of a paragraph boundary' (1982: 123).

Given this research that shows the perceptual reality of paragraphs in both written and spoken language, I conducted the present study in order to discover basic writers' perception of paragraphs. My hypothesis was that basic writers would differ in their perception of paragraphs from other writers--and readers. I assumed that in order to help them improve their writing skills, I must first understand their reading skills; i.e., to understand what they produce, I must first understand what they perceive.

3. Method

In conducting the study, I decided to follow the paradigm established by Koen, et al., (1969). The question I sought to answer was slightly different, however: 'Do basic writers perceive paragraphs similarly or differently from other groups of subjects?' If they did perceive paragraphs differently, I wanted to determine the nature of that difference and any implications those results might have for the development of literacy.

4. Subjects

In order to understand the responses that basic writers would produce in the experiment, I needed to establish a context for their responses. Thus, I selected a total of 7 groups of subjects, representing what I thought to be various degrees of experience with printed text. The 7 groups of readers represented 4 groups of undergraduate students and 3 groups who had graduated from college.

The first 4 groups of subjects were undergraduate students enrolled in different courses within the expository writing program at the Ohio State University. The first group of students consisted of beginning basic writers, enrolled in the first of 2 quarters of basic writing required before they could enroll in freshman composition. The second group were intermediate

basic writers, required to take only one quarter of basic writing before advancing to freshman composition. Students are placed in basic writing courses based on their standardized test scores, typically English ACT scores of 15 or below or SAT Verbal scores of 370 or below, and a writing sample.

The third group of students had enrolled directly in the non-remedial, standard freshman composition course. Such students usually have English ACT scores of 16 through 25 or SAT Verbal scores of 380 through 610.

The fourth group of undergraduates were upperclassmen enrolled in informative writing, an advanced writing course.

The fifth group consisted of students enrolled in their first quarter of graduate study in the Department of English. The sixth group were teachers who were experienced in teaching English language arts in secondary schools and were enrolled in graduate course work in English. The seventh and final group consisted of faculty members in English.

Thirty or more subjects in each group participated in the study. All were native speakers of English.

5. Text

In order to select a text that would be appropriate for the study, I surveyed a number of possibilities, searching for certain characteristics. First, the text should be written in an expository mode of discourse and should be non-fiction rather than fiction, similar to many of the writing assignments made in the expository writing courses in which the undergraduate students were enrolled. While written in the expository mode, the essay should treat a topic of general interest. Its vocabulary should represent a fairly common level of diction, for to the extent that it is possible, the study was not designed to test vocabulary skills.

After surveying many essays, I chose one written by an author who is often anthologized in readers used in writing courses, Lewis Thomas. Thomas, who heads the Sloan-Kettering Cancer Research Center, writes essays on a variety of topics. Several volumes of his essays, which typically first appear in the New England Journal of Medicine, have been published.

The essay selected, "On Death," appears in Table 1. The text, treating a universal topic, consists of 50 sentences arranged in 11 paragraphs. These paragraphs appear in yet a larger, three-part design, consisting of 3, 4, and 4 paragraphs, respectively. The first three paragraphs, sentences 1 through 12, form a discursive beginning that introduces the topic and some of the issues that are discussed later, such as places where death occurs--naturally and unnaturally, reactions to seeing dead animals in public places, and an acknowledgement that death is inevitable and constant, as is life.

The middle section contains 4 paragraphs, sentences 13 through 27, and discusses the natural death of 4 kinds of organisms, each in separate paragraphs: (1) creatures that vanish into their own progeny such as single cells, sentences 13-17; (2) insects, sentences 18-20; (3) birds, sentences 21-23; and (4) animals, focusing on the elephant, sentences 24-27 [1].

The final portion of the essay contains 4 paragraphs, beginning with sentence 28, and presents Thomas's reflections on death.

When presented to the subjects, the essay was double-spaced and typed continuously with only the paragraph indentations removed. As it appears in Table 1, the sentences have been numbered, circled numbers correspond to Thomas' paragraphs, and a # marks the beginning of each line in the version presented to the subjects.

Table 1. Text with Instructions

Instructions: Make a slash / before each sentence which you think begins a paragraph in the following selection.

(1) #Most of the dead animals you see on highways near the cities are dogs, a #few cats. 2 Out in the countryside, the forms and coloring of the dead are #strange; these are the wild creatures. 3 Seen from a car window, they appear #as fragments, evoking memories of woodchucks, badgers, skunks, voles, snakes, #sometimes the mysterious wreckage of a deer. (4) It is always a queer shock--#part a sudden upwelling of grief, part unaccountable amazement. 5 It is simply #astounding to see an animal dead on a highway. 6 The outrage is more than #just the location; it is the impropriety of such visible death, anywhere. 7 #You do not expect to see dead animals in the open. 8 It is the nature of #animals to die alone, off somewhere, hidden. 9 It is wrong to see them lying #out on the highway; it is wrong to see them anywhere. (10) Everything in the #world dies, but we only know about it as a kind of abstraction. 11 If you #stand in a meadow, at the edge of a hillside and look around carefully, #almost everything you can catch sight of is in the process of dying, and most #things will be dead long before you are. 12 If it were not for the constant #renewal and replacement going on before your eyes, the whole place would turn #to stone and sand under your feet. (13) There are some creatures that do not seem #to die at all; they simply vanish totally into their own progeny. 14 Single cells #do this. 15 The cell becomes two, then four and so on, and after a while the last #trace is gone. 16 It cannot be seen as death; barring mutation, the descendants #are simply the first cell, living all over again. 17 The cycles of the slime #mold have episodes that seem as conclusive as death, but the withered slug, #with its stalk and fruiting body, is plainly the transient tissue of a #developing animal; the free-swimming amebocytes use this organ collectively #to produce more of themselves. (18) There are said to be a billion billion #insects on the earth at any moment, most of them with very short life expec-#tancies by our standards. 19 Someone has estimated that there are 25 million #assorted insects hanging in the air over every temperate square mile, in a #column extending upward for thousands of feet, drifting through the layers #of the atmosphere like plankton. 20 They are dying steadily, some by being eaten, #some just dropping in their tracks, tons of them around the earth, disintegrat-#ing as they die, invisibly. (21) Who ever sees dead birds, in anything like the #huge numbers stipulated by the certainty of the death of all birds? 22 A dead #bird is an incongruity, more startling than an unexpected live bird, sure #evidence to the human mind that something has gone wrong. 23 Birds do their #dying off somewhere behind things, under things, never on the wing. (24) Animals #seem to have an instinct for performing death alone, hidden. 25 Even the largest, #most conspicuous ones find ways to conceal themselves in time. 26 If an elephant #missteps and dies in an open place, the herd will not leave him

Table 1. Text with Instructions (continued)

there; the #others will pick him up and carry the body from place to place, finally put-#ting it down in some inexplicably suitable location. 27 When elephants encounter #the skeleton of an elephant out in the open, they methodically take up each of #the bones and distribute them in a ponderous ceremony, over neighboring acres. (28) #It is a natural marvel. 29 All the life of the earth dies, all the time, in the #same volume as the new life that dazzles us each morning, each spring. 30 All #we see of this is the odd stump, the fly struggling on the porch floor of #the summer house in October, the fragment on the highway. 31 I have lived all #my life with an embarrassment of squirrels in my backyard; they are all over #the place, all year long, and I have never seen, anywhere, a dead squirrel. (32) #I suppose it is just as well. 33 If the earth were otherwise, and all the dying #were done in the open, with the dead there to be looked at, we would never #have it out of our minds. 34 We can forget about it much of the time, of think #of it as an accident to be avoided somehow. 34 But it does make the process of #dying seem more exceptional than it really is, and harder to engage in at #the times when we must ourselves engage. (36) In our way, we conform as best we #can to the rest of nature. 37 The obituary pages tell us the news that we are #dying away, and the birth announcements in finer print, off at the side of #the page, inform us of our replacements, but we get no grasp from this of #the enormity of scale. 38 There are three billion of us on the earth, and all #three billion must be dead, on a schedule, within this lifetime. 39 The vast #mortality, involving something over 50 million of us each year, takes place #in relative secrecy. 40 We can only really know of the deaths in our households, #or among our friends. 41 These, detached in our minds from all the rest, we take #to be unnatural events, anomalies, outrages. 42 We speak of our own dead in low #voices, struck down, we say, as though visible death can only occur for cause, #by disease or violence, avoidably. 43 We send off for flowers, grieve, make cer-#emonies, scatter bones, unaware of the rest of the three billion on the same #schedule. 44 All that immense mass of flesh and bone and consciousness will #disappear by absorption into the earth, without recognition by the transient #survivors. (45) Less than half a century from now, our replacements will have more #than doubled the numbers. 46 It is hard to see how we can continue to keep the #secret with such multitudes doing the dying. 47 We will have to give up the #notion that death is catastrophe, or detestable, or avoidable, or even strange. 48 #We will need to learn more about the cycling of life in the rest of the system, #and about our connection to the process. 49 Everything that comes alive seems to #be in trade for something that dies, cell for cell. 50 There might be some com-#fort in the recognition of synchrony--in the information that we all go down #together, in the best of company.

= beginning of a line in the version presented to subjects
(O) = beginning of a paragraph in original text

6. Results

Because I was interested primarily in subjects' responses by groups, I converted the responses for each sentence to percentages for each group, as shown in Table 2, where the number of subjects in each group is also presented. The horizontal lines across the Table correspond to the major divisions within the essay. The results reveal considerable differences

among groups of subjects in the frequency and pattern of their responses, yet there are some similarities as well.

Table 2. Paragraphing of Text by Groups (Percentage Agreement)

	Beginning Basic Writers	Intermed. Basic Writers	Freshmen	Upper-classmen	New Grad Students	English Teachers	Faculty
Number:	44	30	42	33	30	31	31
Sentence No.							
1*	30	27	29	33	50	35	26
2	3	7	10	9	0	3	3
3	0	0	5	0	0	0	0
4*	23	23	19	12	27	10	6
5	9	3	7	18	23	16	26
6	30	30	26	18	17	19	23
7	23	7	17	9	0	0	6
8	11	3	5	3	3	0	0
9	5	3	2	0	0	0	0
10*	36	53	50	52	67	81	74
11	27	17	12	9	17	6	3
12	11	3	0	3	3	0	0
13*	61	80	90	88	87	90	77
14	14	0	2	0	3	0	0
15	0	3	0	0	0	0	0
16	2	0	0	0	0	0	0
17	32	27	33	15	17	10	6
18*	80	73	86	88	67	90	77
19	14	3	2	3	7	0	0
20	0	0	2	0	0	0	0
21*	98	90	88	79	77	81	65
22	0	0	0	0	0	0	0
23	2	0	0	3	0	0	0
24*	50	43	55	67	67	65	58
25	7	7	10	6	3	3	0
26	23	0	5	3	7	0	0
27	7	10	2	0	7	0	0
28*	23	17	10	27	13	10	3
29	66	70	90	67	73	84	84
30	0	0	0	0	0	0	0
31	52	27	33	24	10	10	3
32*	2	3	10	6	20	6	16
33	14	10	5	15	10	3	3
34	14	7	0	6	0	3	6
35	2	0	0	3	3	0	0
36*	34	27	62	58	60	58	68
37	55	27	14	15	23	23	6
38	25	27	19	21	10	6	6
39	2	7	5	9	3	6	10
40	23	13	14	9	7	10	23

Table 2. Paragraphing of Text by Groups (Percentage Agreement) (continued)

41	14	3	0	0	7	3	0
42	25	13	12	9	0	3	3
43	11	7	0	0	0	0	0
44	20	27	21	3	33	19	19
45*	48	30	48	48	33	10	45
46	11	13	24	33	23	23	26
47	14	7	5	3	0	6	0
48	14	20	2	3	0	0	0
49	18	7	7	3	17	6	0
50	25	3	0	0	0	0	0

An asterisk, *, indicates a paragraph opener in the original text.
The horizontal lines indicate the three major sections of the text.

Because one of the primary questions prompting this study was to compare the responses of the seven groups of subjects, I divided the responses to each sentence into four categories of percentages of responses obtained. The first category consists of sentences which no subject indicated as opening a paragraph, sentences that were essentially judged to be paragraph internal. As Table 3 shows, the number of sentences receiving 0% responses increases dramatically. For beginning basic writers, only 5 sentences were not chosen by someone in the group as opening a paragraph. For intermediate basic writers, 9 sentences obtained 0% responses with the number increasing to 11 for freshmen and 12 for upperclassmen. For new graduate students, 16 sentences received 0% responses, with 19 for secondary English teachers and 22 for faculty.

Table 3. Number of Sentences Initiating Paragraphs by Percentage Agreement

	Beginning Basic Writers	Intermed. Basic Writers	Freshmen	Upper-classmen	New Grad Students	English Teachers	Faculty
0%	5	9	11	12	16	19	22
1-34%	37	36	31	31	26	23	21
35-64%	5	1	4	2	2	2	1
65-100%	3	4	4	5	6	6	6

These findings show that beginning basic writers are much more likely to respond to any sentence as a paragraph opener. The increase in number of sentences receiving 0% responses predicts the ordering of groups and is significant at the .0001 level [2].

The second category of sentences represents sentences that approximately one-third of each group of subjects did not select as paragraph openers. The number of sentences receiving 1-34% responses gradually decreases across the groups from a high of 37 for beginning basic writers to a low of 21 for faculty and is significant at the .001 level.

The third group of percentages represents the number of sentences that fell in the guessing range, roughly one-third to two-thirds (35-64%) of each group indicated they opened paragraphs. These numbers range in roughly

decreasing order but are not significant (.119).

The fourth category of sentences are those that obtain strong agreement as paragraph openers--approximately two-thirds or more of the subjects in each group identified them as paragraph openers (65-100%). The number of such sentences increases from 3 for beginning basic writers to 4 for intermediate basic writers and freshmen, to 5 for upperclassmen, to 6 for new graduate students, English teachers, and faculty. This increasing trend is significant at the .005 level.

Five of the 50 sentences in the passage are of particular interest: 3 sentences that 65% or more of the subjects selected as openers and 2 sentences that no subject selected as openers. The 3 sentences selected a paragraph openers are 18, 21, and 29, all occurring in the middle portion of the essay.

Sentence 18 is the sentence that introduces the subject of insects and corresponds with a paragraph opener as written by Thomas. Sentence 21 introduces the subject of birds and also corresponds to a paragraph opener in the original essay. It is a particularly interesting sentence because it received the highest percentage of responses of any of the 50 sentences in the selection, 98% from the beginning basic writers. Not only does the sentence introduce a new topic, it is an interrogative, the only non-declarative sentence in the essay.

The third sentence receiving a high percentage of responses from all groups is sentence 29, which does not open a paragraph in the original text. The preceding sentence, sentence 28, "It is a natural marvel," opens the final portion of Thomas' essay, though for these 241 subjects, it did not. These subjects tended to judge sentence 28 as the concluding sentence of the preceding paragraph, rather than as an opening sentence.

Two sentences in the passage were never selected by any of the subjects as paragraph openers, sentences 22 and 30. Each follows a sentence described above that received a high percentage of responses--the sentence introducing birds and the sentence that opens the concluding section of the essay, as interpreted by the subjects. These responses support in part the hypothesis advanced by Bond and Hayes that 'The length of the current paragraph influences paragraphing decisions' (1984: 159). They predict that 'readers still avoid one-sentence paragraphs' (1984: 165), supported by the results obtained here.

7. Implications

The results of this study reconfirm the psychological reality of paragraphs. For all groups, some sentences achieved high levels of agreement as opening paragraphs. Even for the beginning basic writers, agreement obtained, though only half as frequently as for more experienced readers.

The study also indicates that the nature of the text to be paragraphed influences the nature of responses. Where there are clear shifts in topics and purposes (as in sentences 18, 21 and 29) agreement obtains for all groups of subjects. Where paragraph boundaries are more subtle, only the more advanced groups of subjects will respond, as for sentence 36 which appears in the reflective, final portion of the essay.

The results obtained here also show that responses should not necessarily be categorized as right or wrong as compared to paragraphing in an original text. In this study, sentence 28 received responses in the bottom third, 1-34%, although it opened a paragraph in the original essay, while the following sentence, sentence 29, received responses above 65%, indicating that it began a paragraph.

But the groups of subjects respond in significantly different ways to such a paragraphing task. The differences can be predicted by the apparent experience of each group and indicates that the awareness of paragraphs develops gradually, not suddenly.

Finally, the results show that beginning basic writers do recognize paragraphs in printed texts, but not with as much agreement as their peers and teachers. They perceive text differently, and those differences should influence the instruction they receive and may predict the kind of writing they produce.

Perhaps a final caution should be to those who administer paragraph identification tests. Information about the subjects in such studies is crucial, for in this study, subjects representing different levels of exposure to text produced significantly different results. Whether or not that correlation extends to the identification of paragraph-like units in oral discourse remains to be seen.

Notes

1. Although Thomas discusses birds in a paragraph separate from animals, only 3 groups of subjects responded significantly to the shift in topics; they may have classified birds as animals.

2. I gratefully acknowledge the assistance with the statistical analysis of the data in this study provided by the Statistics Laboratory of the Ohio State University. The reference for the statistical tests used here is Hollander and Wolfe (1971: 222-224).

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Pronouns and People: Some preliminary evidence that the accessibility of antecedents in processing can vary with clause relation and biology*

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The research reported here bears on two distinct complexes of issues in psycholinguistics. The experimental work described below was conceived within an ongoing investigation of certain anaphoric processes that appear to be embedded in the syntactic processing system. From this perspective, the main goal of the work described here was to explore the effect the presence of an antecedent may have on the processing of a pronoun, especially as this is affected by the syntactic relation between the clauses bearing the antecedent and pronoun. The second complex of issues concerns the relation in the brain and mind between the specifically linguistic components of the language processing system (e.g., syntax) and other seemingly more versatile cognitive systems (e.g., those that deal with discourse structure and that interpret utterances against the listener's background of general knowledge). One question of particular relevance to the present study is whether this relation is uniform across individuals.

If the preliminary analyses to be presented here bear up under further scrutiny, it will be evident that these two complexes of questions are intimately intertwined. In brief, in the overall results it appears that an antecedent in the preceding clause can facilitate processing over a span of a few words following a pronoun. This occurs where the relation between the clauses is subordinate-main, but not where the two clauses are coordinate. This general pattern, however, seems to be a reflection of effects that arise in one specific group of subjects, those who have no left-handers among their biological relatives. In those with left-handed relatives the antecedent effect is present regardless of the syntactic relation between the two clauses.

These results are relevant to central theoretical questions about anaphoric processing, the logical architecture of the language comprehension system, and the relation between language and human neurobiology.

1. Background

1.1. Pronominal anaphora within syntactic processing

Much recent linguistic research has suggested that there is an interesting set of syntactic principles bearing on pronominal anaphora (among other phenomena). Within single sentences these principles appear to tightly constrain what pairs of potential antecedents and pronouns must, may or must not be taken to be coreferential (see, for example, Chomsky, 1981, 1986, Reinhart, 1983, Aoun, 1985). Though there are linguists who advocate quite different approaches (Bolinger, 1979, Bosch, 1983, Cornish, 1986), the large body of linguistic work bearing on syntactic aspects of intrasentential pronominal anaphora at least suggests that this area merits some attention in the language processing literature.

Psychological research on pronominal anaphora has been concerned almost exclusively with cases where the pronoun and antecedent are in different sentences (see, for example, Hirst & Brill, 1980, Dell, McKoon & Ratcliff, 1983, Tyler and Marslen-Wilson, 1982, and the review in Garnham, 1985, pp. 148-152). Intrasentential relations have sometimes been examined, but not in ways that exercize the syntactic principles featured in the linguistic literature. For example, Corbett and Chang (1983) used coordinate structures that function as two separate sentences with respect to the binding theory discussed in Chomsky (1981). Garvey and Caramazza (1974) used main/subordinate clause structures that constitute a more integrated syntactic domain, but their research was concerned with semantic influences on reference relations.

The larger investigation of which the present work is a part is designed, among other things, to explore the role of the syntactic processing system in the assignment of reference relations among pronouns and their various candidate antecedents. In particular, it has examined the possibility that some reference relations (or at least some relations that ultimately get interpreted as reference relations) are assigned by the syntactic processor. Previous experimental results indicate that certain cataphoric instances of they can exert an influence on the syntactic analysis of ambiguous gerund phrases (e.g., flying planes), that the reference relations implicated in this finding are assigned even when they result in a manifestly odd or implausible interpretation, that these relations are blocked when they violate syntactic constraints on reference relations, that these relations are unaffected by alternative antecedents in a preceding sentence, and that effects of these kinds are demonstrable with several experimental paradigms (Cowart & Cairns, in press, Cowart, 1986a, 1986b).

The work described here extends this line of investigation to more commonplace instances of pronominal anaphora where the antecedent precedes the pronoun and where a wider variety of pronouns can be investigated. The most basic goal of the work described here was to determine whether a certain variant of the word-by-word reading procedure can detect any indication that pronouns are processed differently according to whether or not an antecedent appears ahead of the pronoun in the same sentence. A second more theoretically significant goal was to determine whether any effects of this kind are sensitive to the syntactic relation between two clauses where the antecedent is in the first and the pronoun in the second. The reference-assigning mechanism that appears to be involved in the cataphoric cases investigated earlier applies, by hypothesis, to third-person pronouns generally (apart from reflexives), and thus should be relevant here. If it is, and it is, as proposed, an essentially syntactic mechanism, it should be sensitive to syntactically significant variations in clause relations.

1.2. Laterality and language processing

There has long been evidence suggesting that the distribution of language-related functions across and within the two hemispheres of the brain is subject to some variation. Though this evidence is difficult to interpret and still the focus of much controversy, it is nonetheless noteworthy that it has had virtually no effect on the bulk of sentence processing research, apart from spotty attempts to control for subject handedness. This

apparently has two causes: 1) it is difficult to assess dominance, and 2) when it is assessed, there is little evidence that it has any effects.

Note, however, that as a point of logic, for intact subjects variation in the distribution of functions should have consequences only where this variation affects the way that various functions interact. Furthermore, there could be variations in the way functions interact that are not very directly related to their distribution.

Recently, Bever, Carrithers and Townsend (1986) reported findings that suggest that more fruitful work on the relation between these matters and sentence processing may be possible. Bever and his collaborators found evidence that some processing phenomena are linked to the presence of left-handers among a subject's biological relatives. For example, in one experiment subjects were asked to indicate whether a probe word heard in isolation shortly after the auditory presentation of a sentence fragment was one of the words in the fragment. Considering only the correct positive responses, subjects who reported no left-handers in their families (hereafter these will be termed 'Right' subjects) were much slower in responding to probes drawn from the latter part of the fragment than they were with words drawn from the earlier part. By contrast, subjects with one or more left-handed relatives ('Lefts' hereafter), showed no serial order effect whatever; the Lefts responded equally rapidly to probes drawn from early or late parts of the fragment and they also responded more rapidly overall than the Rights. Note that all subjects were themselves strongly right-handed. Bever suggests that the performance of the Rights reflects their reliance upon a self-terminating serial search through a linear representation of the utterance just heard. The Lefts, by contrast, are presumed to treat the task by way of a semantic representation that provides simultaneous access to all parts of the context material.

It is, of course, not at all obvious why processing effects of these kinds should be related to the presence of left-handers in a subject's family. Bever's suggestion is that left-handedness is associated with a heritable biological trait that results in a number of neurophysiological consequences. Among these is a richer interconnection between the language processing system, especially its syntactic component, and the balance of the cognitive system, especially those components involved in semantics and interpretation. Thus, the presence of left-handers in a subject's family is merely an index of the likelihood that the subject will be affected by this biological trait. There is an independent line of investigation in neurolinguistics (see, for example, Geschwind and Galaburda, 1984) that seems to lend some credibility to this analysis.

Against this background, the work discussed below was intended to provide a test of Bever's proposals via methods and linguistic phenomena different than those he used. Pronoun-antecedent relations are notoriously subject to a great diversity of influences, ranging from stress to syntactic structure to discourse structure. If the phenomena Bever and his colleagues discovered are related to the degree of interconnection between syntactic and semantic modes of processing, anaphoric phenomena should provide a useful body of experimental material. To the degree that the richness of interconnection between the syntactic and semantic (and discourse) processing

components varies, this should affect the relative accessibility of various approaches to antecedent-finding.

2. Experimental Evidence

Kennedy and Murray (1984) provide evidence that a certain variant of the word-by-word reading procedure is much more sensitive to syntactic structure than were earlier forms of this method. One goal of the present experiment was simply to determine whether this revised procedure can detect effects related to the presence or absence of an antecedent for a pronoun. Secondly, the experiment was designed to manipulate the syntactic relation between the clauses bearing antecedent and pronoun to determine whether any simple antecedent effects that might appear are sensitive to this factor. Finally, the experiment was planned to be run on two distinct samples, a group of strongly right-handed Right subjects and an equally strongly right-handed group of Left subjects.

2.1. Method

The experimental materials consisted of 24 sets of items similar to (1).

(1) a. Even though the librarians had made an awful lot of noise, she kept on working on her own stuff.
b. Even though the librarian had made an awful lot of noise, she kept on working on her own stuff.
c. The librarians had made an awful lot of noise, but she kept on working on her own stuff.
d. The librarian had made an awful lot of noise, but she kept on working on her own stuff.

Note that the second clauses, including their pronoun subjects, are identical throughout, apart from the coordinating conjunction in the (c) and (d) forms. The subject of each first clause is a lexical NP that provides an acceptable antecedent for the pronoun in the (b) and (d) cases only. The pronouns used included *he* and *she*, but *they* predominated. The two clauses of the (a) and (b) cases are in the relation subordinate-main, while those of the (c) and (d) cases are coordinate. A complete listing of the materials together with a summary of the results for each item is available from the author.

The experimental design involved three within-subjects factors, Antecedent (No Antecedent, Antecedent Present), Clause Relation (Subordinate, Coordinate) and Word Position (the position of each stimulus word relative to the pronoun in the second clause). These three factors were crossed by a fourth, History (Right vs. Left subjects, those lacking or having left-handed relatives, respectively).

These materials, together with 48 fillers of diverse kinds, were presented to subjects via a minor variant of the cumulative word-by-word procedure discussed by Kennedy and Murray (1984). In this task the subject must press a key to see each succeeding word in the stimulus sentence on a computer display. The interval between key presses is recorded and serves as a crude measure of reading time per word. Unlike other versions of the word-by-word task, each word is presented one space to the right of the word preceding (apart from line breaks) and stays on the screen until the subject

presses the key following presentation of the last word. Thus the effect is that of seeing a normally formatted text appear one word at a time. A yes/no question appeared after each sentence presentation and the subject responded via a key press. This response was timed, evaluated and recorded, and the subject was given feedback as to the correctness of the reply. When average response time per word went above 550 msec., the feedback message also urged the subject to respond more rapidly.

In preparation for this work, a survey form was distributed to a large number of students in various undergraduate courses at Ohio State University. This form was derived from Geschwind's variant of the Oldfield inventory. It asked for, among other things, information about the handedness of the respondent's biological relatives. Fifty subjects for this experiment were drawn from a pool of about 430 individuals who completed this form. All were strongly right-handed, with laterality scores (using Geschwind's LS) of 90 to 100. Twenty-four had no left-handed relatives and 26 had one or more such relative.

2.2. Results

The results are summarized in Figures 1A and 1B. Note that when an antecedent was present, Right subjects responded faster on the pronoun and the three words following it, but only where the clause relation was subordinate/main. By contrast, with Left subjects the antecedent produced faster responses for several words after the pronoun regardless of the relation between the two clauses. This pattern seems to be reliable.

The principal statistical analyses covered the first three words following the pronoun. The limits of this zone were determined post hoc; it excludes some potentially relevant contrasts on responses to the pronoun itself and to words following this zone but seems on the whole to include effects representative of the overall result. An analysis covering the span running from the pronoun through the fifth word following the pronoun produced similar but somewhat weaker results. For the purposes of this preliminary report effects and interactions that do not seem to be theoretically relevant will be ignored. Extreme response values were reset to +/- 2SD from the subject's mean.

An overall analysis covering results from both subject types produced only inconclusive results. There was an interaction in the by-subjects analysis involving the Antecedent, Clause Relation and History factors, $F_1(1,42)=4.67$, $MSe=1638$, $p<.05$., $F_2(1,22)=1.2$, NS, as well as a main effect for the Antecedent factor, $F_1(1,42)=7.68$, $MSe=1929$, $p<.01$, $F_2(1,22)=3.02$, $MSe=4644$, $p<.1$. The interaction supports the view that the included two-way interaction between the Antecedent and Clause Relation factors is different for Right and Left subjects.

The strongest statistical evidence for a contrast between the performance of Right and Left subjects appears when analyses are restricted to just one of these groups at a time. For the Right subjects the Antecedent by Clause Relation interaction is highly significant, $F_1(1,20)=8.89$, $MSe=1357$, $p<.01$, $F_2(1,22)=8.78$, $MSe=1470$, $p<.01$, indicating that the apparent contrast between the effects of the Antecedent factor in the two Clause

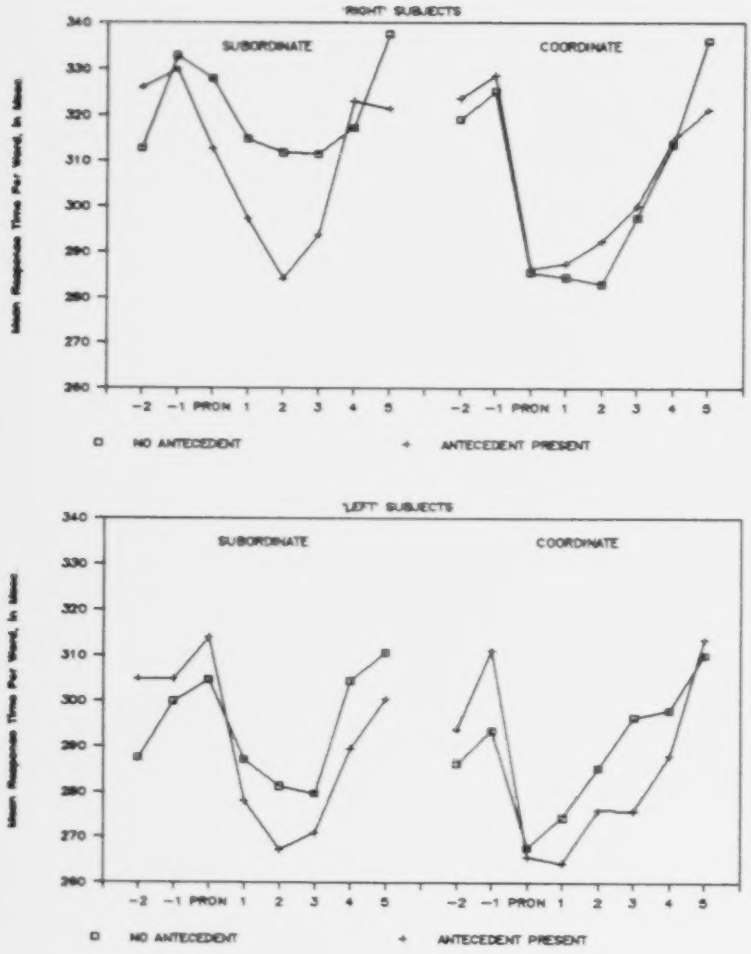

Figure 1A & 1B. Mean response time per word for Right subjects and Left subjects as a function of 1) the presence or absence of an antecedent, 2) the syntactic relation between the two clauses, and 3) word position relative to the pronoun ('PRON').

Relation conditions is reliable. In the Left subjects, this same interaction does not approach significance, $F_{1,2}<1$.

On the other hand, the main effect of the Antecedent factor is significant in the results for the Left subjects, $F_1(1,22)=5.64$, $MSe=1949$, $p<.05$, $F_2(1,22)=4.64$, $MSe=2092$, $p<.05$, indicating that the antecedent speeded responses generally, without regard to the relation between the clauses. For the Right subjects, this main effect falls well short of significance, $F_1(1,20)=2.42$, $MSe=1907$, $p>.1$, $F_2(1,22)<1$.

Pilot studies as well the present experiment suggest that one reliable distinction between Right and Left subjects is that the latter generally respond faster. Though this contrast (the History main effect) is not significant in the by-subjects analysis, it is highly significant in the by-sentences analysis (where it is treated as a within-'subjects' factor), $F_1(1,42)<1$, $F_2(1,22)= 25.7$, $MSe=1900$, $p<.001$. Comparing the four Right subject cells at each of eight word positions with the corresponding four Left subject cells shows that the Right subjects were slower in 30 of 32 comparisons, $p<.001$.

These results support two important conclusions. First, there is some antecedent-finding mechanism that can influence performance when an antecedent for a pronoun is available in a prior clause that is syntactically integrated with the one bearing the pronoun. Second, effects attributable to such a mechanism are apparent only with subjects who have no left-handers among their close biological relatives.

3. General Discussion

Pronouns are important from several points of view. Questions about how pronouns are associated with their antecedents define one of the central problems in the theory of discourse processing. These questions bear quite directly on the general organization of the language comprehension system, especially questions about 1) how the diverse kinds of information involved in language comprehension are brought to bear on an incoming utterance, and 2) how the results of diverse analyses are integrated. This in turn can be seen as a special case of the complex of problems in the philosophy of mind that have recently been discussed under the heading of modularity theory (Fodor, 1983).

To properly determine pronoun-antecedent relations, listeners must employ many different kinds of information. Some of the kinds of information used are clearly syntactic, but most are semantic or have to do with discourse structure or knowledge of the world. Modularity theory is consistent with only certain possible accounts of the interface among these various kinds of knowledge. Strictly speaking, the linguistic system is modular in Fodor's sense, so long as there is an informationally-encapsulated parser, regardless of how the syntactic aspects of pronoun-antecedent relations are handled. Nevertheless, there are ways to handle syntactic constraints on pronoun-antecedent relations that would be a serious embarrassment to modularity theory. Suppose that a putatively autonomous syntactic processing system is put in harness with a discourse processing system that, together with various sorts of semantic and discourse analyses, computes c-command relations in the course of assigning antecedents to

pronouns. The question would naturally arise as to why other aspects of syntactic analysis might not also be undertaken by this system, thus making the autonomous syntactic processor at least partly redundant. If modularity theory is generally correct, a more consistent outcome would seem to be that an inventory of the capacities of the syntactic processor exhausts the syntactic capacities of the listener, and further, that (conscious reasoning aside) listeners have no capacity to handle syntactic relations apart from what is implemented in the syntactic processing system.

Within this framework, the interface problem for pronouns takes this form: how can the syntactic constraints on pronominal anaphora be implemented without compromising the uniqueness of the various processing subsystems, especially the syntactic processor? Of course, whatever solution is proposed here must respect the fact that for only a relatively small proportion of all pronoun instances will syntactic constraints uniquely and definitively determine an antecedent.

These considerations seem to allow several different ways to organize the interaction between syntactic and discourse processing. One would be for the syntactic processor to add a table to the syntactic representation of each sentence that specifies all possible syntactically acceptable coreference relations within that sentence (cf., Jackendoff, 1972). Another possibility is for the syntactic processor to propose some specific network of coreference relations within each sentence, thus resolving sentence-internal ambiguities. This set of relations is then evaluated by the discourse processor, which has the capacity to revise many of the relations posited by the syntactic processor. The inverse must also be considered; it could be that the syntactic processor makes no assignments of its own, but only evaluates those made by the discourse processor. This would apparently require that there be some mechanism by which it might 'insist' on certain relations, as with reflexives and reciprocals.

The evidence reviewed here suggests that the second of these possibilities is the better model for Right subjects. The large Antecedent effect in the Subordinate condition indicates that something like a reference relation is being assigned, but the extreme sensitivity of this effect to variations in the syntactic relation between the clauses suggests that the mechanism that produces it is essentially syntactic; it seems unlikely that any mechanism that evaluates prospective antecedents in terms of their plausibility or reasonableness in the discourse would be so dramatically sensitive to this sort of syntactic variation. Since these subjects can, presumably, still take the NP in the first clause as the antecedent of the pronoun by later application of discourse processes, these processes seem to be positioned to receive an input from the syntactic processor with some reference relations already specified.

The results for the Left subjects reveal less about the interface between syntactic and discourse processing. The uniformity of the Antecedent effect clearly shows that the mechanism that produces it in these subjects is less sensitive to syntactic structure than is the mechanism controlling the performance of Right subjects. This, however, does not preclude the possibility that some relations are assigned by a syntactic mechanism; it might be that for these subjects the syntax-based assignments are more readily supplemented by those produced by the discourse processor. It does

seem clear, however, that a discourse-oriented mode of processing is at least more influential for these subjects than it is for the Right subjects.

The general question about the difference between Right and Left subjects will likely be hard to resolve. Bever (1986) seems to suggest that for Left subjects syntactic and interpretive processing are more intimately integrated, but that these subjects' capacity for syntactic analysis is no less developed than it is in Right subjects. Richer interconnection between syntactic and interpretive modes of analysis simply makes the interpretive modes more salient cognitively and more influential in behavior. Detailed demonstrations of syntactic influences on Left subjects may, however, be difficult to provide.

Though much further research is required, it is clear that the results reported here bear on the two sets of issues raised in the introduction. There does seem to be a syntax-based mechanism for assigning something like a coreference relation. There do seem to be biological differences between subjects that affect the way various modes of language processing are integrated.

Notes

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Some Implications of Issues in Social Dialectology for Linguistic Reconstruction*

Robert J. Jeffers

1. Introduction

1.1. There has long been a tendency in work on linguistic reconstruction, in particular in the field of Indo-European studies, to frame questions relating to properties of prehistoric grammars solely in terms of the particular constituents, construction types and categories that occur in the descendant historical languages. So, for any construct, construction or category that occurs in one/some of the extant languages of a family, scholars typically seek to establish whether it existed in the parent language; similarly, if different forms/structures have corresponding functions in related languages, scholars commonly attempt to determine which of the alternatives is more archaic, presuming that it will most closely reflect the prehistoric situation. Are plain velars, ablative case endings, future tense forms, morphological infinitives, relative pronouns, subordinate clauses reconstructible for proto-Indo-European? Questions of this sort have occupied the attention of Indo-Europeanists for more than a century.[1]

The result of this methodological bias has often been to limit the structural parameters within which the general character of prehistoric grammars might be conceived, and to subtlely distort the role of the principle of uniformitarianism in reconstruction. That well-established principle demands that prehistoric grammars manifest only those structural properties occurring in known languages; it does not, however, presume that a prehistoric grammar be expected to share structural/typological properties with the grammars of its particular attested descendants.

In calling attention to this methodological bias, I do not intend to contest the self-evident fact that the actual grammars of attested languages must serve as the basic data for reconstruction; my purpose is rather to emphasize the fact that reference to information of all sorts about the nature of linguistic systems can prove relevant in the construction of our hypotheses about the character of prehistoric grammars by offering new perspectives/contexts in which to interpret those basic data.

In this regard, consider how reference to the discoveries of linguistic typology has informed recent investigations into the nature of the early Indo-European phonological system. Some of the most enlightening and encouraging work of the last decade in this area has been generated by hypotheses (grouped under the general rubric "the glottalic theory") that attribute to prehistoric Indo-European an obstruent system that, though natural and well represented among the world's languages, is nowhere attested in the Indo-European language family.[2]

1.2. Important research of the last two decades which concentrates on language in its social context supports the claim that correlations obtain between certain structural properties of language and the sociolinguistic context in which language is used (and undergoes change). For the most part,

however, this interesting work in social dialectology has exerted little influence among linguists concerned with the reconstruction of prehistoric grammars and with the identification of actual processes which account for the historical grammars which serve as the bases for our hypotheses about prototype languages. It is my purpose in this paper to call attention to the potential relevance of certain aspects of this research for work in linguistic reconstruction. Specifically, it is suggested here that reference to the probable sociolinguistic circumstances in which early Indo-European was originally spoken and subsequently changed might offer insights relevant to some of the more intransigent problems in Indo-European historical grammar, particularly in the area of syntax.

Section 1 of the paper considers the potential relevance for reconstruction of recent investigations supporting a distinction between autonomous and non-autonomous language; section 2 considers some implications of recent studies of language shift in progress in multilingual speech communities.

2. Autonomous versus non-autonomous language

2.1. Based on a study of English social dialects, Basil Bernstein (1974) introduced into the sociological literature the notions "restricted code" and "elaborated code" for the speech styles of British working class and middle class young men, respectively. (The more recent term variety is surely to be preferred to speech style for Bernstein's categories, as they refer to social dialects, not socially determined registers.) As the result of subsequent research by scholars investigating other aspects of the relationship between linguistic structure and social context, this early and somewhat unfortunate dichotomy between elaborated and restricted codes has been, for the most part, superseded by a distinction between autonomous language and non-autonomous language (e.g., Ray 1977).

The newer terminology emphasizes what is essentially a typological difference between varieties of language typical of oral-mode/context-sensitive communication, on the one hand, and text-mode/context-free communication, on the other. Whereas Bernstein's elaborated/restricted code distinction was meant to reflect a relationship between linguistic structure and the linguistic capacities of particular, socially definable groups of language users, the autonomous/non-autonomous distinction properly calls attention to the relationship between linguistic structure and the communicative context and function of language itself. It is also now clear that text-mode varieties of language do not replace oral mode varieties in some inevitable evolutionary progression, as assumed in some early discussions of this phenomenon; rather, "the two are superimposed upon and intertwined with each other" (Tannen 1982). In light of these additional considerations, sociolinguistic situations of the sort originally described by Bernstein demand a more complex analysis. It seems quite likely, for example, that at least some of the differences that he identified are properly to be understood to reflect the consequences of a sort of dialect contact phenomenon. While the everyday speech of the middle class youths might well manifest the consequences of extensive contact with the autonomous language of the standardized English grapholect (Haugen 1966, Ong 1982),[3] the speech of the working class youths would not.

The identification of the distinction (actually, the continuum) between autonomous and non-autonomous language has far-reaching consequences for linguistic typology. For, if communicative context in some way/degree shapes the formal properties of language, we should expect to see cross-linguistic and cross-varietal differences that correlate with the distinct functional demands of autonomous versus non-autonomous language. In fact, studies of creoles, enclave languages, and at least some languages of non-literate cultures tend to support the hypothesis that the grammars of languages/varieties which are restricted to use in context-sensitive situations share certain structural properties - properties which correspond in kind to those characteristic of oral-mode communication, in general?

A brief consideration of so-called "enclave languages" may serve as an instructive example at this point. In her 1985 dissertation, Julianne Maher establishes the notion enclave language. An enclave language is actually a variety (commonly, a dialect) of a language whose speakers are isolated in time and/or space from contact with speakers of a standard variety of that language which reigns/reigned as the language of the "establishment" and of literacy in another speech community. It is the native language of a group which does not represent the establishment in a multi-lingual community, and hence is used by the minority group only in domestic, singularly oral-mode contexts. Louisiana French is an example. Typically, most members of the linguistic minority in an enclave speech community are bilingual speakers of the enclave language and of the language of the establishment.

Maher identifies certain structural properties which are characteristic of enclave languages on a cross-linguistic basis. These enclave features include: phonologically invariant morphemes; analytic (as opposed to synthetic) forms/constructions; rigid word order; a focus on aspect in the verbal system; equivalent interrogative and relative forms/constructions; and inter-clause syntax characterized by adjoined (as opposed to embedded/incorporated) clauses. These properties generally differentiate the enclave varieties from their sister dialects, but cannot typically be attributed to influence from the contact (establishment) language. Hence, it appears that these shared structural characteristics must be associated with the sociolinguistic and functional properties shared by these languages. What is perhaps of even more general interest and relevance is the fact that many of the same structural properties identified as characteristic of enclave languages are among those generally associated with other non-autonomous/oral-code (as opposed to autonomous) linguistic systems.

Consider the possible relevance of these discoveries concerning the structural properties of language associated primarily with context-sensitive communication situations for the reconstruction of the grammars of prehistoric and preliterate speech communities. Is it not presumptuous, for example, to assume that the grammar of early Indo-European should, in some trivial way, have the "look" of a "typical" or "classical" Indo-European language, like Sanskrit or Greek? Is it not, in fact, possible (even, likely) that the grammar of the Indo-European speech community would exhibit structural properties of the sort commonly encountered in oral-code linguistic systems in general.

I should reemphasize here that this exhortation to practioners of linguistic reconstruction (in particular, to Indo-Europeanists) to expand the frames of reference deemed appropriate in our conjurings on prehistoric grammar (in particular, on early Indo-European grammar) in no way implies any demotion in importance of the structural facts of the extant languages. To be sure, a principal (perhaps, the principal) goal of reconstruction is a coherent and plausible diachronic account of the structural facts of the several grammars of the extant members of an alleged "family of languages", constructed in terms of some hypothesis about a unitary source. The exercise of reconstruction is, in effect, an attempt to make explicit the nature of the relationship that obtains among genetically associated languages through identification of the separate evolutionary routes connecting each of the extant grammars to their common prehistoric ancestor. The methodological principle at issue here is that our hypotheses about the source should be informed - to the degree possible - by any relevant facts about the nature of linguistic systems. For, as the validity of that hypothesis (i.e., the reconstruction) is strengthened, the quality of the diachronic account of the structural facts of the several reflections of that source must improve.

1.2. In the following paragraphs I propose to suggest some implications of the issues discussed in section 1.1. for the reconstruction of a particular aspect of early Indo-European grammar, specifically inter-clause syntax.

Most traditional scholarship on the subject of inter-clause syntax in Indo-European represents a search for comparative evidence to support the presence (or absence) of "subordinate clauses" in the parent language. As I have remarked elsewhere (Jeffers 1986), "it would only be a slight exaggeration to describe the history of the study of pIE inter-clause syntax as a series of attempts to answer the famous question "Gab es im Indogermanischen Nebensätze?""

Edward Hermann's 1895 article with that question as its title served as the starting point for discussions of inter-clause syntax in early Indo-European for decades. Hermann concluded that the comparative data offered no grounds for the reconstruction of morphological or lexical markers of subordination, a conclusion supported by many of the most distinguished Indo-Europeanists working in the first half of this century.[4] This conclusion had profound implications for the study of Indo-European syntax for decades. It will be useful here to quote somewhat extensively on this subject from my recent paper on methodology in syntactic reconstruction (Jeffers 1986).

> It is important to recognize that a profoundly important corollary was assumed to follow from the conclusion that morpho-syntactic markers of subordination are not reconstructible for the parent language - that corollary being that early IE syntactic structure was characterized by an almost absolute version of parataxis. If the grammar of prehistoric Indo-European cannot be shown to include subordinate constructions of the sort that typically occur in extant IE languages, then - the argument goes - pIE grammar must have been destitute of formal devices that mark syntactico-semantic relationships between clauses. Delbrück, in fact, asserts .... "that there was once a time in which only principal clauses

(Hauptsätze) existed" (1900:412). However, the very notion principal/main clause has meaning only with reference to some corresponding and particular notion of non-principal clause; though treated as universally applicable categories by Delbrück and his successors, references to these complementary notions clearly reflect an acquaintance with particularg grammars (the grammars of IE text languages) that were seen to manifest, though sometimes incorrectly, ... a particular structural dichotomy between so-called Haupt- and Nebensätze.[3]

In light of what we know about the structural properties of language associated with strictly oral-code modes of communication, we might reasonably add an alternative, or at least additional, context within which the facts about inter-clause syntax in early Indo-European might be reviewed. Recall that one of the typical features of enclave languages and of other language associated with context-sensitive, oral-mode communication situations is a system of inter-clause syntax characterized by adjunction, as opposed to embedding and/or incorporation. The term adjunction characterizes systems of inter-clause syntax in which the related clauses retain their internal structural integrity and surface structure autonomy, but are marked as as members of a larger syntactic structure by some morphological, lexical or syntactic device.[4]

Several recent papers (C. Lehmann 1980, Holland 1984, Jeffers 1986) call attention to the fact that a careful analysis of the surface syntactic structures of the most ancient representatives of Indo-European (Anatolian, and the varieties of Ancient Greek and Indo-Iranian encountered in the texts of the oral traditions) support the hypothesis that embedding/incorporation is not a feature of early Indo-European syntax. Jeffers 1986, for example, includes a review of the full range of situations in which a reflex of the Indo-European particle *yo plays some role in marking a relationship between two clauses. A few instructive examples drawn from that paper follow; these examples from Vedic Sanskrit correspond to relative, adverbial, and predicate complement constructions in the later languages. Note that in all cases, each of the two clauses maintains its internal structural integrity and surface structure autonomy.[7]

(1) adjoined relative.

yam bhadreṇa śavasā codayāsi prajāvatā rādhasā/
Whom w/blessed w/might you quicken w/children w/wealth

te syāma (RV 1.94.15)
they may we be

(2) adjoined relative of purpose.

tat savitur vareṇyam bhargo devasya dhīmahi/
that of S. desirable glory of god we attain

dhiyo yo naḥ pracodayāt (RV 3.62.10)
thoughts our stimulates

"May we attain that desirable glory of the god S. which (so that it) may stimulate our thoughts."

(3) condition.

sadyaś cid yaḥ sahasrāṇi śatā dadāśa
someone who thousand 100 gives

sakir ditsantaṃ ā minat
no one the-one-wanting-to-give (would) restrain

(4) cause.

scittī yat tava dharmā yuyopima/
unknowing bec. your law we have disturbed

mā nas tasmād enaso deva rīriṣaḥ (RV 7.89.5)
not us fr/that fr/sin god you harm

"Do not here because of that sin, because unknowing(ly) we disturbed your law."

(5) ?result/purpose with inflected form.

indrāgnī yuvaṃ su naḥ sahantā dāsathaḥ rayim/
I & A you surely us mighty will give wealth

yena..... sāhiṣīmahi (RV.10.1)
so that/whereby we may overcome

(6) result with lexical conjunction.

gṛhān gacha/ gṛhapatnī yathā asaḥ (RV 10.85.26)
house go mistress so that you may be

(7) predicate complement (precursor).

gṛṇe tad indra te śava upamaṃ devatātaye/
I praise this Indra your prowess highest for gods

yad dhaṃsi vṛtraṃ ojasā (RV 8.62.8)
that you strike V. w/might

The question of the earliest function of the erstwhile particle *yo is of particular interest here. *yo is commonly referred to as the "relative particle" (see however Gonda 1954), but appears in Indo-European languages as 1)the stem of inflected relative words, 2)the stem of a wide variety of lexical conjunctions, 3)a clitic sentence connective in Hittite (see Watkins 1963) and also 4)in the genitive case ending *-syo.

The Hittite reflex of *yo is -ia (alternant -a).[8] This Hittite particle, which is generally referred to as a sentence connective, can be functionally distinguished from a distinct group of IE "sentence connectives" which occur clause initial or as clitics (i.e., *no, *wo, *to, *e/o ; Hittite nu-, wa-, ta-, Luvian a-/-a). The forms in this latter group function as discourse particles and simply move the narrative forward; they often occur sequentially in a string of four or five clauses. In sharp contrast, the Hittite form -ia/-a (which, as has been noted, is cognate with the stem of relative words and conjunctions of other IE languages) connects two clauses in which the described actions or states are intimately connected in time and space (i.e., where the clauses describe two components of a single situation) or where the connected clauses refer to parallel notions.[9]

The Hittite particle -ia/-a is clearly not a relative word. Likewise, although they are frequently termed relative conjunctions in the literature, the various conjunctions in other ancient IE languages formed on the stem *yo are not proper relatives. In fact, it is only in the cases where we see an inflected form of *yo (a relative pronoun/adjective) that coreference is a factor in the inter-clausal relationship;[10] and in these forms the semantic information relevant to coreference is carried by the case affixes whose "attachment" to *yo may well reflect a secondary reanalysis and restructuring, the details of which remain obscure.

In light of these and other related facts (see Jeffers 1986), a reconsideration of of the earliest function of the particle *yo seems to be in order. It appears that the one property common to all occurrences of the ancient particle *yo in the earliest texts - and which is therefore potentially reconstructible for early Indo-European - is its function as a marker of a relationship between two structurally autonomous clauses which must be interpreted as constituents of a larger syntactic construction, i.e., between a pair of adjoined clauses.[11] Note that this etymological analysis of *yo substantiates the syntactic evidence from early texts (exemplified in (1)-(7) above) supporting the hypothesis that early Indo-European grammar was characterized by, or at least comprehended, adjoined clauses as a feature of inter-clause syntax.

Consider now how this discussion demonstrates that reference to "external" but relevant facts about the nature of linguistic structure can serve to inform a reconstruction. Such reference may present an alternative context for the generation of hypotheses about the structural properties of the source language - (if you know that adjunction is something of a commonplace in language associated with context-sensitive situations, it becomes an obvious/possible candidate for status as a structural property of the language of a pre-literate speech community) - or, alternatively, it may offer "extra-familial" support for a relatively speculative hypothesis about the source language which is based on limited, ambiguous or otherwise difficult to interpret data in the extant languages - (the evidence for an early IE syntactic system with adjoined clauses is preserved in relic constructions in the earliest, pre-classical texts of the ancient IE languages; a diachronic account centered on this evidence is rendered less speculative/tentative, if considered with reference to types of syntactic systems not typical in the IE family, but relevant on other - usually typological grounds).

It follows from this discussion that much of the traditional and more recent work on inter-clause syntax in early Indo-European can be viewed as a misdirected effort. The preoccupation with a search for evidence supporting (or not supporting) the presence of typically Indo-European subordinate (especially, relative) clauses becomes a pointless exercise, because "by definition" it precludes from consideration any alternative systems for marking inter-clause relationships.[12]

3. Language change in the multi-lingual speech community

Historical linguists have traditionally made reference to two types of language change, internal and external. Internal change results from some structural disequilibrium within a language, which exerts pressure for change. External change results from outside influences, and language contact is commonly viewed as the "cause" of this sort of change.

Some students of language change, such as Schuchardt in the nineteenth century and Bloomfield in the twentieth, have held that language contact can have a profound effect on the structure of languages. However, Meillet is representative of most nineteenth and early twentieth century historical linguists (Indo-Europeanists, in particular) in the assertion that the influence of languages upon each other is seldom extensive, certainly not to the point of "mixed systems" that defy genetic classification. Sapir, of course, believed in the natural resistance of language to external influence, and Jespersen agrees with Whitney that the essential nature of language remains unaltered by contact with other languages. A most consistent theme, moreover, in almost all early discussions of language contact and language change is that syntax is the component of grammar most resistant to contact-induced change. Somewhat surprisingly, this assumption persists even in some contemporary investigations of language contact speech communities. Karttunen, for example, states that "syntax remains most resistant to change" in American Finnish (1977:183), even after detailing several significant syntactic replacements.[13]

In recent years, the emphasis of research on language contact has shifted away from retrospective analyses of borrowing to studies of the actual linguistic behavior of speakers in multilingual speech communities. Some studies concentrate on the social correlates of linguistic choices, especially in situations where a language shift is in progress (e.g., Gal 1979). Others investigate the implications for linguistic structure of bi- and multilingualism.

The results of research on the structural implications of bi- and multilingualism suggest a few things that must be taken into account by practicing reconstructionists. Since the publication of Weinreich's breakthrough study Languages in Contact (1953), it has been clear that the interference phenomena that are the product of language contact cannot always be predicted on the basis of the structural properties of the interfering language. It is often the case that a wholesale rearrangement of patterns may result from the intrusion of some new forms or patterns.

The recent work on enclave languages referred to in section 2.1 supports the claim that the sorts of innovations that affect languages in contact

situations can be profound and that they are not necessarily restricted to the neat incorporation of some "foreign element" into the inventory of forms and patterns of the borrowing language. Likewise, consider the extensive recent studies of creolization (e.g., Bickerton 1981, Sankoff 1980).

Alternatively, it appears that under certain sociolinguistic conditions adult speakers do not "(bring) forth ... novel devices for coping with a new language," but call upon "methods of dealing with ill-fitting material that were inherent in their native language .... they (deal) with masses of material in rational ways that they (bring) with them" (Karttunen 1977:174). Karttunen's somewhat impressionistic characterization might be reformulated in terms of the abductive-deductive model of language change originally explicated in Andersen 1973.

When language learners (chidren or adults) are confronted with perceived ambiguities in forms and constructions, they are forced to guess at the structure of a grammar that might produce such structures by means of an abductive inference. In such situations, we should not be surprised to find that speakers sometimes opt for a grammatical analysis for the ambiguous surface structure which happens to be consistent with that of obviously related forms where the structural analysis is unambiguous. We call that process analogy. Harris (1984) suggests that typological harmony plays a similar role in directing language change, once innovation is likely or inevitable. He characterizes phenomena like analogy and the tendency toward typological harmony as "gutters" that serve as pathways of least resistance, but which are in no way deterministic or causal (see also Jeffers 1985:252-53).

Some of the products of language contact in multilingual communities may also be understood in terms of this model. It seems quite reasonable that the rules/principles of the native grammar of a bilingual should play a similar role in his/her attempts to attach a grammatical analysis to actual language data of a second language. Many of the distinctive syntactic patterns of Irish English, for example, most likely reflect restructurings of this sort. (E.g., "I'm just after going"; "It's Sean that's going to Dublin"; etc. On Irish-English see Bliss 1972, 1977, 1979.) The Irish/English contact situation seems also to have produced novel constructions for Irish English, which cannot be explained in terms of restructuring produced by a straightforward reanalysis of English language data in terms of the principles of Irish grammar. See, for example, Kallen 1986 on "The co-occurrence of do and bí in Hiberno-English".

Whether contact-induced change results in novel structures characteristic of neither contact language, or in restructurings that are the products of reanalysis of language data of one language in terms of the grammatical rules/principles of a second (i.e., the language learner's first) language, it now seems clear that contact situations can produce dramatic rearrangements of linguistic structure in one or a very few generations. The traditional claim of historical linguists about the natural resistance of languages to external change is quite simply not supported by actual studies of bi- and multilingual speakers, or of speech communities experiencing some sort of language shift. As Vincent points out in a study of the results of Celtic/English bilingualism in Ireland, England and Wales, "As far as syntax is concerned (emphasis RJJ),

there is growing evidence from second language learning and bilingualism that grammatical interference is rampant between source and target language and between dominant and non-dominant language" (1984:166).

Consider for a moment the implications of these claims about the diachronic consequences of language contact for investigations into the prehistory of the Indo-European language family. It must be assumed that the several Indo-European dialects represent, to a significant degree, products of the sorts of processes at work in contemporary multilingual speech communities. The early Indo-Europeans moved across Europe and Western Asia into existing speech communities representing a wide variety of indigenous languages. Consequently, we must at least entertain the hypothesis that many of the radical structural (even typological) differences that we confront within the Indo-European language family, in syntax and in other areas of grammar, reflect the sorts of massive disruptions that can occur in multilingual speech communities. As the IE parent language must have served as the target language in a wide variety of language shift situations, it seems entirely reasonable that many radical structural discrepancies among the descendants of early Indo-European, (such as verb-final properties in Indo-Aryan alongside verb-initial properties in Celtic) may be attributable to mechanisms of change similar to those that produced both novel and Celticized syntactic patterns in Irish, Cornish and Welsh English (Vincent 1986).

Students of linguistic prehistory should take heed. Much of the frustration generated by recent work on syntactic reconstruction (see, e.g., Lightfoot 1980) may result from attempts to construct hypotheses about the source language based on a narrowly defined notion of what constitutes a plausible diachronic account of an extant language. Studies of language shift and of the actual dynamics of language contact in bi- and multilingual speech communities clearly demonstrate that efforts to account for the diachronic relationships between a hypothetical source and its several extant reflexes cannot depend solely on the search for genuine correspondences and grammar internal motivations for change in syntactic systems, or, for that matter, in any other area of grammatical structure.

Notes

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1. There have, of course, been some important exceptions. Ergativity and agglutination, for example, have been proposed as properties of early or pre-Indo-European.

2. The several versions of the glottalic theory, which was initially explicated in Gamkrelidze and Ivanov 1972 and Hopper 1973, share the feature that glottalized stops replace the voiced unaspirated stops of the traditionally reconstructed system of obstruents. Several systematic anomolies can be accounted for by means of this reconception of early Indo-European phonology.

3. Some commentators do argue that the actual thought processes of members of highly literate speech communities are structured by the technology of writing, i.e., by their command of autonomous language. See, e.g., Olson 1980; Ong 1982.

4. Hermann admits the possibility that subordinate clause marking by means of accent is reconstructible for proto-Indo-European. Also, Meillet (1937) and others do point out that there is some evidence for reconstructing a class of non-finite verbals (see however, Jeffers 1976; Jeffers & Kantor 1984), and substantial evidence for reconstructing participles.

5. The last decade has witnessed the advent of the typological method (TM) for syntactic reconstruction in Indo-European studies, championed primarily by Winfred Lehmann (e.g., Lehmann 1974). This approach is much more open to positing for the parent language syntactic structures which are substantially different from those found in the extant languages. But, investigations within the framework of TM are also constrained by tacit assumptions about the range of structural devices for marking inter-clause syntax that represent viable candidates for reconstruction. The preoccupation with word order typology as a framework for the reconstruction of prehistoric syntax introduces into the process a new set of typologically based predispositions which may or may not be relevant in a particular case. Proponents of TM pose questions of the following sort (Lehmann 1980) to frame issues in syntactic reconstruction: "Does the evidence of the extant languages support the reconstruction of preposed or postposed relative clauses?" The options for early Indo-European are thereby reduced to one of two possibilities. This clearly represents a misguided approach to syntactic reconstruction, most notably because it disregards the fact that (incorporated) adnominal relative clauses are not absolute universals of language. (For additional discussion see Jeffers 1986.)

6. The earliest and most frequently cited characterization of adjunction is the discussion of multi-clause sentences in Australian in Hale 1976. Consider the following examples from Walbiri (after Hale 1976), in which the form kutja- (prefixed to AUX) marks the inter-clausal relationship. In a., the two clauses share a coreferential noun phrase; in b., they do not.

a. yankiri-li kutja-lpa ŋapa ŋa-ṇu ŋatjulu-lu ∅-na pantu-ṇu.
emu-ERG COMP-AUX water drink-Past I-ERG AUX spear-Past
"While the emu was drinking water, I speared it." or
"I speared the emu that was drinking water."

b. ŋatjulu-lu lpa-ŋa kaḷi tjaṇṭu-ṇu, kutja-∅-npa ya-nu-ṇu ṇjuntu.
I-ERG AUX boomerang trim-Past COMP-AUX walk-P-hither you.
"I was trimming a boomerang when you came up."

7. Additional examples from Sanskrit, as well as corresponding constructions from Homeric Greek and early Latin are given in Jeffers 1986.

8. For a more complete discussion of the etymology of Hittite -*in*, see Watkins 1963; see also Jeffers 1986.

9. See Jeffers & Pepicello 1979 and Jeffers 1986 for a more complete discussion of the functional distinction between the Hittite particle -*in*/-*a* and other sentence particles in Hittite and Indo-European.

10. Note further that inflected reflexes of **yo* in Vedic Sanskrit and Homeric Greek do not, in fact, introduce embedded relative clauses of the sort familiar from the later classical languages (Note exx. (1) and (3) above). They function as topicalizers, announcing that a particular noun will be a predicate in the next clause. See Holland 1984 and Jeffers 1986 for additional discussion.

11. Compare this interpretation of the original function of the IE formative *yo* with the corresponding function of the Australian particle *kutja*- described by Hale and referred to in footnote 4 of this paper.

12. Much of the work on reconstruction of inter-clause syntax within the framework of the typological method (see fn. 5) confronts a similar problem. For example, an analysis of relative clauses that is constructed to determine whether there is evidence to support the reconstruction of prenominal or post-nominal relative clauses becomes an exercise in frustration, if the grammar of the source language turns out to be one that does not comprehend incorporated constructions of any sort.

13. Note, however, that the innovations in American Finnish and corresponding structures in Russian and Swedish Finnish do not reflect direct structural influence from the contact languages. They appear to exemplify developments of the sort described in Maher 1985 for enclave languages.

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The phonological domains of final lengthening

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1. The phonological framework

A syllable at the end of a phrase is considerably longer than it would be phrase-internally. Similarly, a stressed monosyllable is often observed to be longer than any segmentally identical syllable that is separated from the word's edge by one or more unstressed syllables. This paper describes some experiments we conducted in an attempt to determine the precise domain of these effects. Relying on earlier reports of other seemingly related phenomena (e.g., Gee & Grosjean 1983), we assume that the domain is phonological; although surface syntactic constituency influences the effects, its influence is mediated by prosodic structure. By way of introduction, therefore, we first review the potentially relevant prosodic domains.

We consider the prosodic structure of an utterance to be a hierarchical arrangement of various prominence-lending phonological properties. This arrangement can be represented by a metrical grid with suitable bracketings at any level that also has constituents with phonologically marked edges. The grid below, for example, represents the phrase phonological structure as it might be said in isolation, with an intonation typical of citation forms. (We adopt the intonational analysis and notation of Pierrehumbert 1980.)

```
[              x    ]      nuclear accent, boundary tone
     x         x           accent
  x  x         x           stress
  x x x x x    x  x        syllable
phonological structure
      |          |
      H*         H* L L%
```

The lowest level of this grid consists of seven local sonority peaks defining events called 'syllables'. Three of these syllables contain unreduced vowels, and are qualitatively longer and louder than the others, properties which define another level of events called 'stresses'. Two of these stressed syllables are autosegmentally associated to certain prominence-lending tonal configurations in the intonation contour, the two H* 'pitch accents'. The association to a pitch accent creates another level of prosodic strength, that of 'accented syllables'. The last pitch accent is followed by an unassociated L tone, the 'phrase accent'. The falling tonal pattern created by the juxtaposition of the phrase accent helps to give the syllable associated to the last pitch accent a special prominence known as 'nuclear stress' or 'sentence stress'. (The last accent itself is designated the 'nuclear accent'.) There is also a L% 'boundary tone' aligned to the edge of the phrase after the phrase accent. This boundary tone phonologically marks the end of a constituent called the 'intonational phrase'.

A fact to note about this grid is that only the highest level corresponds to any well-documented phonological constituent. Here, there is a boundary tone to mark the edges of units headed by nuclear stresses, whereas every other level only has the phonological event marking the prominence peak. An attractive hypothesis, therefore, is that phrase-final lengthening is merely the durational correlate of the boundary tone, and thus is limited to syllables at the ends of intonational phrases.

2. Intonational phrasing

We tested this hypothesis using the sets of sentences shown in Table I. The first set had a three-way contrast among pep, pepper, and peppermint, in which an identical stressed target syllable is separated from the end of the word by 0, 1, or 2 unstressed syllables. The second set had a similar two-way contrast between Pop and Poppa. It also had different verbs following the target nouns so as to keep a constant inter-stress interval length. Both corpora also contrasted pairs of sentences in which the material following the target either is or is not a kind of clause that is obligatorily set off as a separate intonational phrase. One subject read the 'pep-pepper' corpus and two subjects read the 'Poppa posed' corpus. They read the sentences from a randomized list for a total of five tokens of each type at each of three different self-selected speaking rates. The readings took place in a sound-treated recording booth, and the recorded sentences were analyzed using a digital waveform editor. (The same methods were used for the subjects in all subsequent experiments described below.)

Fig. 1 shows the overall results from the 'pep-pepper' experiment averaged over all three rates. The target syllable was nearly twice as long in pep as in the other two words, but only in the sentences where the word boundary coincided with an obligatory intonational phrase break. An analysis of variance showed significant main effects for word and for phrasing, and also a significant interaction between the two variables ($F=2,75$, $P<0.0001$). These results suggest strongly that the domain of any phrase-final lengthening is the intonational phrase.

However, one aspect of the data in fig. 1 seems to contradict this hypothesis. Although the difference was not as large as in the sentences with the obligatory intonational break, the vowel in pep was significantly longer even in the no-break condition ($F=2,16$, $P<0.0001$).

Table I. Corpora for intonational phrasing experiments

1. a. Pep, for the lack of which the party will suffer, is not to be had.
   Pepper, for the lack of which the chili will suffer, is not to be had.
   Peppermint, for the lack of which the frosting will suffer, is not ...
   b. Pep for the party is not to be had for love or money.
   Pepper for the chili is not to be had for love or money.
   Peppermint for the frosting is not to be had for love or money.

2. a. Pop opposed the question strongly, and so refused to answer it.
   Poppa posed the question strongly, and then refused to answer it.
   b. Pop, opposing the question strongly, refused to answer to it.
   Poppa, posing the question strongly, demanded an answer to it.

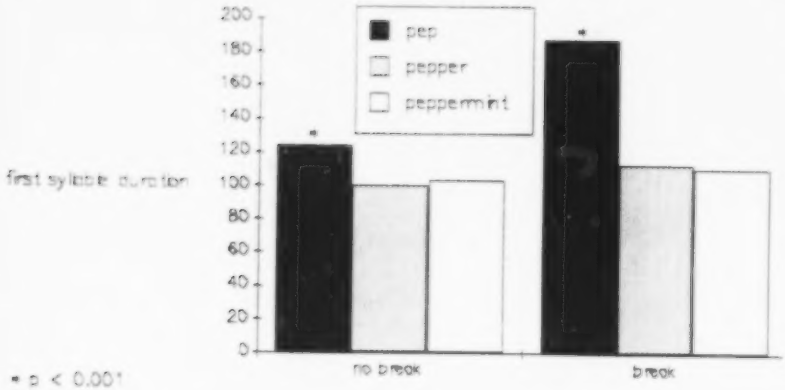

Figure 1. Mean durations in ms. for vowel in first syllable in 'pep-pepper' experiment averaged over all rates. Subject RWF.

The results for the 'Poppa posed' corpus were similar. Both subjects had a considerably longer [a] in Pop and schwa in Poppa in the sentences with an obligatory intonational phrase boundary following the target word, and both subjects also showed differences that were smaller but in the same direction for the sentences with the other syntactic structure. For subject JRE the smaller differences were significant overall, while for subject LAW they were significant only when separate ANOVA's were calculated for the different rates, and then only for the slow rate, as illustrated for the [ɒ] in fig. 2.

Our first thought on seeing the smaller difference in the sentences with no obligatory medial break was that the subjects must have produced optional breaks in some tokens of these sentences. The interaction with rate for subject LAW in the 'Poppa posed' corpus made this explanation seem especially likely, since speakers tend to produce more intonational phrases when they speak more slowly or more deliberately. When we listened to these utterances, and looked at their fundamental frequency patterns, however, we saw no evidence of such a drastic restructuring. We concluded that there is a real smaller effect in these sentences which is different from the substantial phrase-final lengthening at the intonational phrase boundary. We would like to think that the smaller effect is also some sort of final lengthening, perhaps for a constituent at some lower level of the grid. Since none of the other levels have independently motivated phonological constituents, however, we must first consider another possible explanation that does not involve positing a phonological phrase smaller than the intonational phrase.

English is often claimed to be stress-timed, with stressed syllables following each other at regular intervals. In strong versions of the stress-timing claim, such as that of Pike 1945, this rhythmic regularity is purportedly achieved by adjusting segment durations when different numbers of

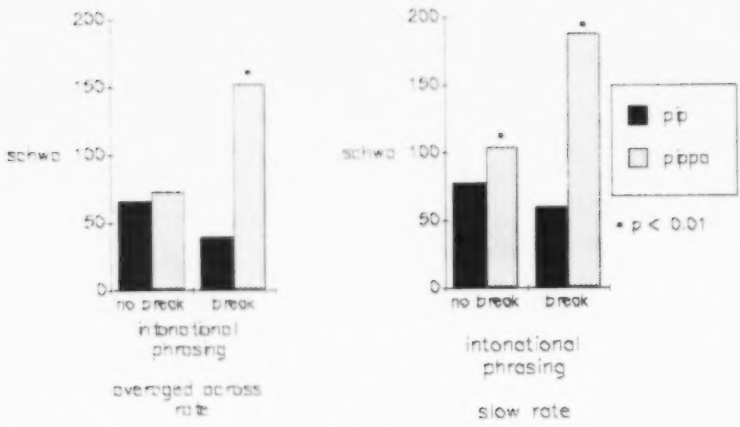

Figure 2. Mean durations for schwa in 'Poppa posed' corpus averaged over all rates (left) and for slow rate only (right). Subject LAW.

unstressed syllables intervene between stresses. Thus, the results from the 'pep-pepper' corpus could be evidence for stress-timing rather than any indication of the existence of phonological constituents smaller than the intonational phrase, as illustrated by the grids below, in which vertical lines separate the inter-stress intervals:

x x pep ..	x x x for the	x x x party	vs	x x x pepper	x x x for the	x x x chili

But stress-timing cannot explain the 'Poppa posed' corpus results, since in that corpus there was always exactly one unstressed syllable in the interval between the stress in the target noun and the stress in the following verb. Therefore, the smaller difference in the sentences where there was no medial intonational phrase break must be a final-lengthening effect and a boundary mark for some smaller phonological constituent. We labeled the effect 'word-final' (as opposed to 'phrase-final') lengthening, and did two further experiments in order to locate it more precisely in relation to the grid.

3. Accentual phrasing

The first hypothesis we considered is that word-final lengthening is a boundary mark for a constituent that is the domain of the pitch accent. This seemed a likely possibility, because accents belong to the intonation, whereas stress patterns are largely specified in the lexicon. Also, speakers may produce more pre-nuclear pitch accents in slower renditions of a given sentence, a tendency which could explain the rate effect in subject LAW's results. We therefore posited the existence of 'accentual phrases' headed by accented syllables and bounded by word-final lengthening, as shown below:

```
[                x     ]   nuclear accent/intonational phrase
[     x     ] [  x     ]   accent/accentual phrase
phonological structure
      |           |
      H*          H* L L%
```

We first tested this hypothesis with the sentences in Table II, which again contrasted the phrases Pop opposed and Poppa posed. Before saying each target sentence, the subjects read a context question, which induced contrastive focus either on the noun or on the following verb. Contrast puts nuclear stress on the item in focus. Thus, depending on the context question, there would either be nuclear accent on the verb and a medial accentual phrase boundary coinciding with the target word boundary, or there would be nuclear accent on the target noun and only one accentual phrase in the sentence. Since we assumed further that everything in an utterance belongs to some accentual phrase, we thought that the phrase containing the nuclear accent

Table II. Focus placement corpus for accentual phrasing experiment

1. a. Q. So, your dad liked the question?
   A. Pop OPPOSED the question.
   b. Q. So, your dad answered the question?
   A. Poppa POSED the question.

2. a. Q. So it was grandpa who opposed the question?
   A. POP opposed the question.
   b. Q. So it was grandpa who posed the question?
   A. POPPA posed the question.

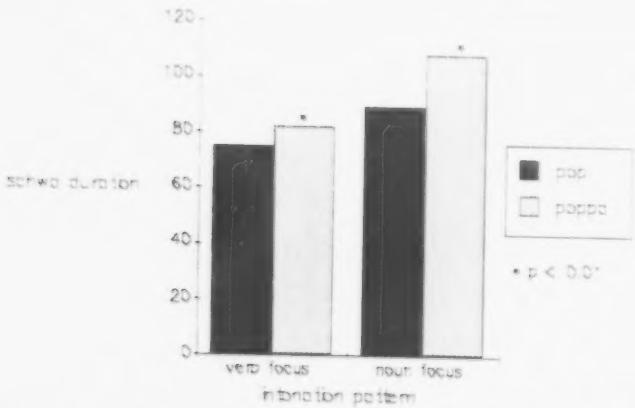

Figure 3. Mean durations for [a] in focus experiment sentences produced at normal rate. Subject JRE.

must continue to the end of the sentence, predicting that the answers with focus on the noun would differ from those with focus on the verb by showing no word-final lengthening on the target words.

This prediction was not borne out. The same two subjects who read the 'Poppa posed' intonational phrasing corpus also read the focus corpus. Fig. 3 illustrates the results by showing the mean values for the schwa in the sentences at normal rate produced by subject JRE. The word-final schwa in Poppa posed was significantly longer than the non-final schwa in Pop opposed whether the focus was on the verb or on the noun. The results for the preceding [a] for this speaker and for both vowels for the other speaker are similar. Thus, in terms of the prediction, this experiment does not support a unit at the level of accents as the domain for word-final lengthening.

On the other hand, these results constitute evidence against the hypothesis only if everything in an utterance must belong to some accentual phrase. But if only syllables in words with accents belong to constituents at this level, the results are equivocal. The lone accentual phrase in the sentences with focus on the noun would then terminate at the end of the target word and the following material up to the end of the sentence would be unaffiliated to any accentual phrase, as illustrated below:

```
[x                    ]   nuclear accent/intonational phrase
[x  ] _________________   accent/accentual phrase
 x     x         x        stress
 x  x  x      x  x  x     syllable
POPPA posed the question.
 |
 H*          L         L%
```

(In this grid, the underscore at the accent level highlights material that is unaffiliated to any accentual phrase.) The focus sentences thus might give evidence for the accentual phrase, but they could not disprove it.

4. Accentual phrase, stress foot, or independent prosodic word?

Table III gives sample sentences from the experiment that we designed to correct this flaw of the corpus involving contrastive focus. The target phrases in this experiment, superstition, super station, and Sioux perspective, all have the same stress pattern but different word-boundary placements. The sentences also contrasted three different intonation patterns chosen for their pitch-accent placements relative to the two stressed syllables in the target phrases. In the first pattern, the nuclear accent is on make, so that there can be no accents on either stressed syllable in the target phrase because it is in post-nuclear position. The second pattern placed 'scooped' L*+H accents on the word real preceding the target phrase and on the second stressed syllable in the target phrase, but no accents on the first syllable. The third pattern placed a pre-nuclear L* accent on the first stress and a nuclear H* on the second stress in the target phrase.

This corpus tests three hypotheses about word-final lengthening. The first is again the notion that the lengthening marks accentual phrases. The test for this hypothesis is that, since a lexical item can have more than one accent, there should be word-final lengthening internal to lexical items when

Table III. Intonation patterns for second accentual phrasing experiment

1. post-nuclear
   You may call it a superstition, but that doesn't MAKE it a superstition.
   H* L L%

2. uncertainty contour
   Q. Do you have any feigned beliefs?
   A. I have a real superstition.
   L*+H L*+H L H%

3. surprise-redundancy contour
   Don't you understand?! It's a superstition!
   L* H* L L%

---

accents are placed appropriately. Thus, superstition should pattern exactly like super station; its [u] should be shorter and its schwar longer than in Sioux perspective, but any difference among the three phrases should hold only when both stressed syllables are accented, in the surprise-redundancy contour:

```
[x  ] [ x   ]   [x  ][ x   ]   [ x ] [    x    ]   accentual phrases
 x      x        x     x         x       x
 x x    x  x     x x   x  x      x    x  x  x
super station   superstition   Sioux perspective
 |      |        |     |        |       |
 L*     H*       L*    H*       L*      H*
```

The second hypothesis is that word-final lengthening marks a 'stress foot'. If this hypothesis is correct, then there should be the durational patterns just described, but without the dependency on accent placement:

```
[x  ] [ x   ]   [x  ][ x   ]   [ x ] [    x    ]   stress feet
 x x    x  x     x x   x  x      x    x  x  x
super station   superstition   Sioux perspective
```

The third possibility is that phrasing below the intonational phrase level is independent of the prosodic hierarchy, that the word-final lengthening marks a 'prosodic word' that is not necessarily headed by any prosodic peaks such as accents or stresses. In this case, final lengthening should occur only at the edges of actual lexical items, so that the schwar in superstition should always be shorter than that in super station:

```
[   ] [     ]   [          ]   [   ] [         ]   prosodic words
super station   superstition   Sioux perspective
```

We had six subjects in this experiment, and the results showed two different patterns. For the first speaker, the [u]'s in superstition and super station were shorter than in Sioux perspective, but only in the suprise-redundancy contour, where they were accented as well as stressed (fig. 4a). The [ɚ] in superstition also patterned like that in super station (fig. 4b).

In both words, it was consistently longer than in Sioux perspective, but again, only in the suprise-redundancy intonation. The similarity of superstition to super station and the dependency on accent pattern for any difference among the words suggests that the relevant unit for word-final lengthening is an accentual phrase.

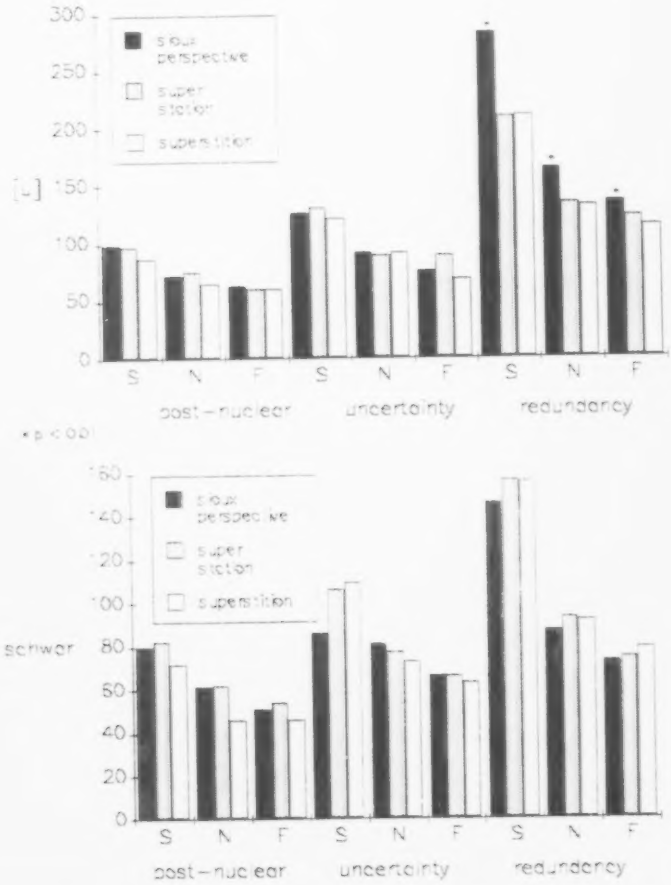

Figure 4. Mean durations for [u] (top) and [ə] (bottom) in each test phrase, averaged by rate (Slow, Normal, Fast) and by intonation pattern. Subject JRE.

The second speaker, on the other hand, showed no dependency on the accent pattern. He had a longer [u] in Sioux regardless of the intonation pattern, although it was consistently so only at the slow rate (fig. 5a). His second syllables also showed no dependency on accent (fig. 5b). The schwa in super station was longer than in Sioux perspective whatever the accent placement,

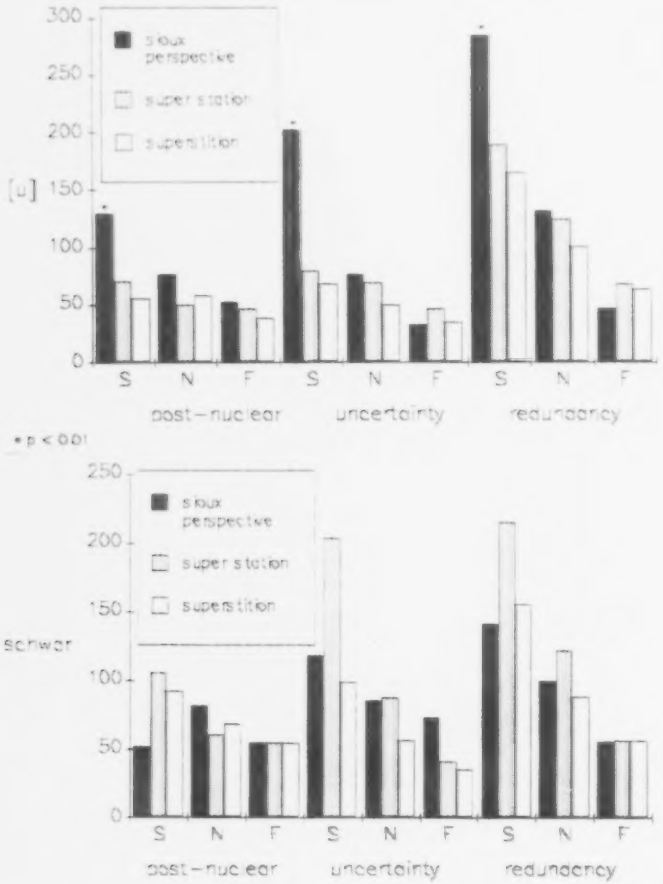

Figure 5. Mean durations for [u] (top) and [ə] (bottom) in each test phrase, averaged by rate (Slow, Normal, Fast) and by intonation pattern. Subject JSC.

although again only at the slow rate. Moreover, superstition did not pattern like super station. Instead, its schwar was generally shorter, like the non-final vowel in Sioux perspective. Thus, this subject's results do not support either the accentual phrase or the stress foot as the domain for word-final lengthening. They suggest rather a prosodic word that is independent from the hierarchy of stresses and accents.

Of the other subjects, two seemed to pattern like the first, showing some evidence for the accentual phrase, and two patterned more like the second, showing evidence for the prosodic word as a phrasal constituent that is independent of the prosodic hierarchy of stresses and accents. The comparisons which support these apparent patterns did not often reach significance, however. The insignificance of the differences in relation to the measure of error in the statistical analysis is perhaps inevitable given the small size of the word-final lengthening effect and the small sample sizes of the categories being compared. (Recall that each of the bars in figs. 4 and 5 represent only five tokens). Thus, since few of the crucial comparisons reached significance, these results do not argue conclusively for two possible speaker-dependent patterns in the use of word-final lengthening.

On the other hand, our experiments do sustain two important conclusions. First, they strongly suggest that there are two different final-lengthening effects: phrase-final lengthening and word-final lengthening. Phrase-final lengthening occurs at intonational-phrase boundaries, and is a large effect that is highly consistent across speakers and rates. Word-final lengthening occurs at some smaller constituent's boundaries, and is a much smaller effect that is not always discernible in experiments that have only five tokens of each type. Second, the word-final effect cannot be explained as a result of stress-timing in English and must be a true final lengthening. However, more ambitious experiments are needed to locate its domain more precisely below the intonational phrase.

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On Situation Adverbs

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In previous work, William Lycan (1984) and I (in Geis (1973, 1985, 1986a, 1986b), have developed a syntactically and semantically motivated theory of conditional sentences in which it is claimed that pairs like (1a) and (1b) have essentially the same logical forms.

(1) a. I will leave if you leave.
b. I will leave in any circumstance in which you leave.

On the semantic side, we have argued that if-clauses involve restricted universal quantification over situations or circumstances (cf. Geis (1973) and Lycan (1984)). On the syntactic side, we have argued that if-clauses are a species of free relative clause and are syntactically quite like the adverbial relative when-clause of (2).

(2) I will leave when you leave.

Documentation of the syntactic similarities between sentences like (1a) and (2) is provided in Geis (1985).

In Geis (1986a, 1986b), I suggested that conditional and certain other types of adverbials are instances of what I called "situation adverbs." Their function is to identify the situations or circumstances in which actions or states of affairs obtain. Thus, in (1a), the if-clause identifies a situation in which the speaker's leaving will obtain, this situation being that the hearer leave. In this paper, I would like to discuss some of the special features of situation adverbs and to discuss how situation and temporal adverbials interact.

Situation Adverbials

The paradigm cases of situation adverbials are conditional adverbials such as those in (3), which are hypothetical situation adverbials, and concessive adverbials such as those in (4), which are factive situation adverbials.

(3) a. I will leave if you leave.
b. I will leave only if you leave.
c. I will leave even if you leave.
d. I will leave unless you leave.

(4) a. I will leave although I don't want to.
b. I will leave despite the fact that I don't want to.
c. I will leave even though I don't want to.

I say that concessives are factive because sentences containing complex concessive adverbials entail the propositions expressed by the subordinate concessive clause. Thus, all of (4) entail

(5) I don't want to leave.

The semantic similarity between (3c) and (4c) is especially close. One's intuition is that they are minimal pairs differing only with respect to the semantic property of factivity (or its opposite, hypotheticalness).

Multiple Situation Adverbials

One of the most interesting properties of conditional clauses is that more than one can occur in a given clause. Consider, for instance, such sentences as (6).

(6) a. If John's car won't start, I will drive you home if my car doesn't break down.
b. If John's car doesn't start, I will drive you home unless it snows.

Moreover, both conditional and concessive clauses can occur as members of a given clause. Consider (7).

(7) a. Although I don't want to take you to work, I will do so if my car doesn't break down.
b. If your car doesn't break down, mine probably will even though it is brand new.

And, multiple concessive clauses also occur together, as is shown by (8).

(8) a. Although I didn't want to leave, John asked me to even though he knew that he shouldn't.
b. Despite the fact that I was told not to, I applied for the job even though I didn't think I had a chance to get it.

Multiple occurrences of a given type of adverbial in a single clause are, in general, impossible unless they form

what I shall call a "semantically nested construction." Note that in a hovel refers to a location in Boston in (9).

(9) John lives in a hovel in Boston.

One can form paraphrases of in a hovel in Boston in which the nesting is made quite explicit: in a hovel which is in Boston. And, in general, if more than one locative adverbial occurs in a given clause, the result will be a nested locative construction.

Sentences containing multiple temporal phrases and clauses are tricky because temporal adverbials can be interpreted as situation adverbials in certain linguistic contexts. Let us begin by noting that two instantaneous time adverbials cannot occur together in a clause:

(10) *John left at noon at five.

If two time adverbials occur in a given clause, one will normally refer to a time or interval within the interval referred to by the other, as in (11).

(11) John left at noon on Friday.

The sequence at noon on Friday is clearly a semantically nested temporal construction. Semantic nesting occurs even when the time adverbials are not contiguous. This is true of the temporal adverbials of (12).

(12) a. On Friday, I will leave at noon.
b. I will next week leave at noon.

There are apparent counter-examples to the claim that multiple occurrences of temporal adverbials in a single clause are nested. Consider (13).

(13) When will you leave at noon?

There are two logically possible ways in which one can take (13). One possibility is that when refers to an interval within which the noontime in question occurred, i. e., when...at noon is a semantically nested construction. The other possible interpretation of (13) is that when refers to some occasion or circumstance on which John left at noon. On this view, (13) has an interpretation something like (14).

(14) On what occasion will you leave at noon?

Note that both of the questions (13) and (14) could be answered by either of the following sentences:

(15) a. On Friday.
b. When his father was in town.

In (15a), on Friday is an explicit temporal adverbial, but the when-clause of (15b) could as likely refer to a situation as a time. In my view, the more plausible interpretation of (13) is the latter one, in which when is construed as a situation adverbial. The question arises as to why some temporal adverbials can be used as situation adverbials.

As these data make clear, some temporal constructions, especially temporal pronouns like when (cf. (13) and then and when-clauses (cf. (15b)), can function as situation adverbials, albeit nonstandard ones. Such interpretations are forced when they occur with explicit temporal adverbials in circumstances in which semantic nesting is not possible.

Note that each of the when-clauses of (16c) is consistent with the main clause I will leave, but the result of combining them is very strange.

(16) a. When you leave, I will leave.
b. I will leave when Mary wakes up.
c. *When you leave, I will leave when Mary wakes up.

One might squeeze out an interpretation of (16c) by construing when you leave as a situation adverbial, i. e., one that refers to some occasion or situation. Nevertheless, it should be clear that having two when-clauses in a given clause leads to a much less acceptable sentence than does having more than one genuine situation adverbial. As a result, we must view temporal clauses as highly marked situation adverbials. How, exactly, we are to account for this is something of a mystery.

Though it must be conceded that the facts surrounding multiple occurrences of temporal constructions in a single clause are cloudy, one generalization holds true: whenever we have two explicit temporal constructions they comprise a semantically nested construction or one of them will be interpreted as a situation adverbial. We may conclude, then, that we do not get two or more non-nested, semantically independent, explicitly temporal adverbials in a given clause.

Multiple Occurrences of Situation and Temporal Clauses

It is possible to mix temporal and concessive clauses in the same clause. However, when such a situation does occur,

the temporal clause is clearly in the scope of the situational clause. I believe that if neither of the included clauses of the following pairs of sentences are read appositively, then the better sentence is one in which the conditional adverbial is outside the scope of the temporal adverbial:

(17) a. I leave for work when my wife does unless it snows.
b. *I leave for work unless it snows when my wife does.
(18) a. I will leave for work before you do if it snows.
b. *I will leave for work if it snows before you do.

The same seems to be true of mixes of temporal and concessive clauses:

(19) a. I will leave for work when you do although I suspect it will rain.
b. *I will leave for work although I suspect it will rain when you leave for work.

Semantically, it is quite clear that the temporal clause is inside the scope of the concessive clause in an elliptical sentence such as (28).

(20) I will leave for work when you do although I told Bill I wouldn't.

Sentences (20) clearly has the same meaning as (21).

(21) I will leave for work when you do although I told Bill I wouldn't leave for work when you do.

Clearly, the phrase leave for work when you do is in the scope of the concessive clause in these sentences. Note further that the if-clause is outside the scope of the modal will in sentence (22).

(22) John will leave tomorrow if we ask him to do so tonight.

This provides further evidence that situation adverbs are outside the scope of temporal constituents in main clauses.

It would appear, then, that we are justified in thinking that temporal constructions are in the scope of situation adverbials. The question arises as to how to account for

this. Suppose that we say that the function of time adverbials (and tense) is to date dateless state-descriptions, i. e., a dateless description of a state of affairs or action. We might formalize a sentence like (23a) as in (23b).

(23) a. John died at noon.
b. (∃t)(At(Die(John), t) & (t = noon) & EarlierThan (t, now))

It is important to recognize that the output of the temporal operator At is different from its input. The nonrecursiveness of temporal adverbials, including temporal clauses, we could say is the result of the fact that their input must be undated state-descriptions.

Why are multiple conditional clauses possible, when multiple temporal or locative clauses are not? Let us say that a situation is a state of affairs or action. Such sentences as (24) all describe situations.

(24) a. John kissed Mary.
b. John will marry Mary.
c. John plans to divorce Mary.

Thus, (24a) refers to a situation in which John kissed Mary, (24b) to a future situation in which John marries Mary, and (24c) to a (more or less continuous) situation in which John plans to divorce Mary. Notice that the sentences of (24) all entail the corresponding sentences of (25).

(25) a. John kissed Mary in some situation.
b. John will marry Mary in some situation.
c. John plans to divorce Mary in some situation.

Thus, there is good reason to believe that ordinary dated state-descriptions refer to situations. It would appear from this than (24), no less than (25), refer to situations.

Following Lycan (1985), we might formalize (26a) as in (26b).

(26) a. I will leave in some situation.
b. (∀ s)(In((∃ t)(At(I leave, t) & EarlierThan(t, now))), s)

In this sentence the function of the conditional adverbial in some situation is to situate a dated state-description, i. e., relativize it to a situation. In a sentence like (27) we

are being told that John's marrying Mary will obtain in a situation in which Mary asks him to marry her.

(27) John will marry Mary if she asks him to.

We might say, then, that this sentence will be true in any future circumstance in which Mary asks John to marry her and he does marry her. Let us notate this as in (28).

(28) (∀s)(In(Mary asks John to marry her, s) ---> In(John will marry Mary, s))

This sentence in turn refers to a class of situations in which John's marrying Mary is linked to her asking him to. Since this is itself a state-description, it can serve as the input to the operator *if*, as in (29).

(29) If his parents will permit, John will marry Mary if she asks him to.

We may notate this as in (30).

(30) (∀s_1)(In(John's parents permit him to marry Mary, s_1) ----> In((∀s_2)(In(John will marry Mary, s_2) ---> In(John will marry Mary, s_2), s_1))

Since s_1 itself refers to a situation, we could in principle relativize it to some additional situation, say, the situation in which John has enough money to buy a house, as in (31).

(31) If John comes up with the money to buy a new house, he will marry Mary if she asks him to if his parents will permit him to do so.

Though (31) is not the most natural sentence, it strikes me as grammatical. Certainly it is quite clear in meaning.

Abstractly, we can represent cases of multiple occurrences of conditional clauses as follows:

(32) a. (∀s_1)(In(S, s_1) ----> In (P, s_1))
b. (∀s_2)(In(S, s_2) ----> In((∀s_1)(In(Q, s_1)-
---> In (P, s_1)), s_2))
c. (∀s_3)(In(S, s_3) ----> ((∀s_2)(In(R, s_2)---
-> In((∀s_1)(In(Q, s_1) ----> In (P, s_1))),
s_2), s_3))

Clearly, this process is recursive, allowing indefinitely

many conditional constructions to occur in a given clause. On this view, then, temporal constructions do not iterate because the output of the temporal operator *at* is different in type from its input. On the other hand, conditional constructions do iterate because the output of the operator *in* is the same in type as its input.

We are now in a position to explain how it is that *when*-clauses can be construed as conditional clauses. Dated state-descriptions cannot serve as the input to the temporal operator. A sentence like (33) is dated and cannot be re-dated.

(33) John left at noon.

Thus, if a temporal adverb like *when* is added to this sentence, as in (34), it must be construed as performing other than a dating function.

(34) When did John leave at noon?

In such a case, it functions as a situation adverb. When this is not possible for pragmatic or semantic reasons, as is true of (35), which suggests that John may have died more than once, the sentence is pragmatically unacceptable.

(35) When did John die at noon?

Why, though, is *when* construed as a situation adverb in a sentence like (39), as opposed to something else? I would suggest that the reason is that times are crucial individuators of situations. Thus, John's leaving at noon is a different event from his leaving at midnight. I would suggest that the use of a time adverb to refer to a situation is metonymic in character, for as noted, the time at which a situation obtains is a crucial part of the make-up of a situation.

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On the Phrasing of Coordinate Compound Structures[1]

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1. Amphibology Resulting from Binary Coordinate Compounding

Coordinate compounding provides a notoriously rich set of possibilities for amphibology (structural ambiguity), as the following example illustrates.

(1) Bill and Ilse or Chuck

Example (1) is felt to have the interpretations of the unambiguous examples (2)-(3).

(2) either Bill and Ilse or Chuck

(3) Bill and either Ilse or Chuck

The difference between these two interpretations cannot be attributed to differences in meanings in any of the words in (1); hence it must, according to widely accepted views, be attributed to a difference in structure, and more particularly to a difference in phrase structure. Figure 1 presents the rules of a simple phrase-structure grammar that generates (1) and that associates with it distinct structural descriptions that correspond to the readings in (2) and (3).

(a) NP --> NP CNP

(b) CNP --> CRD NP

(c) NP --> NOUN

(d) NOUN --> <Bill | Chuck | Ilse | ...>

(e) CRD --> <and | or>

Figure 1. Rules of a simple phrase-structure grammar for coordinate compounding of NPs in English.

[1] Earlier versions of this paper were presented at New York University, October 14, 1986; the 1986 NYSCOL meeting at SUNY/Albany, October 26, 1986; and at CUNY Graduate Center, December 19, 1986.

The structural descriptions that the grammar in Figure 1 associates with the string in (1) are diagrammed in Figure 2 and Figure 3.

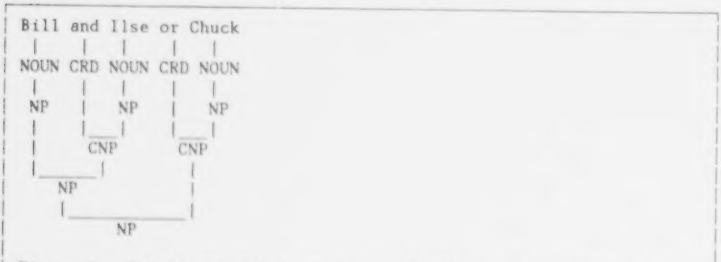

Figure 2. The structural description of (1) with respect to the grammar in Figure 1 that corresponds to the reading (2).

Figure 3. The structural description of (1) with respect to the grammar in Figure 1 that corresponds to the reading (3).

The number of structures associated by the grammar in Figure 1 with phrases consisting of n conjoins grows exponentially with n.[2] Figure 4 presents the number of structures associated with phrases with up to 10

[2] We follow Quirk and Greenbaum (1973) in using the term 'conjoin' to refer to the phrases that are ultimately connected by a coordinating particle. We reserve the term 'conjunct' to refer to conjoins connected by and and 'disjunct' to refer to conjoins connected by or.

conjoins. The progression in Figure 4 consists of the Catalan numbers which can be computed by means of the formula in (4).[3]

(4) $C(n) = (2n-2)!/n!(n-1)!$

It is easily determined that the ratio of two adjacent Catalan numbers approaches 4 in the limit; that is, the progression grows by slightly less than the power of 4. This result is typical of the 'combinatorial explosion' in degree of amphibology predicted by simple phrase-structure grammars.

Number of conjoins	Number of structures
2	1
3	2
4	5
5	14
6	42
7	132
8	429
9	1430
10	4862

Figure 4. Number of structures associated with coordinate compound phrases generated by the grammar in Figure 1 as a function of the number of conjoins.

2. Amphibology Resulting from Unbounded Coordinate Compounding

The coordinate compound structures that the grammar in Figure 1 generate all have exactly two conjoins per constituent. However, coordinate compound structures in natural languages may have any number of conjoins per constituent greater than one. For example, the string in (5) may be understood as having the 'flat' structure shown in Figure 5, as well as nested structures that correspond to those in Figure 2 and Figure 3 with the word and substituted for the word or.

(5) Bill and Ilse and Chuck

The interpretation of (5) corresponding to the structure in Figure 5 is that of a group of three individuals; the other interpretations are those of a group made up of an individual and a subgroup of two individuals, with varying identification of the individual and the members of the subgroup.

[3] I thank Slava Katz for the formula in (4). The corresponding formula in Church and Patil (1982: 141) actually computes the values of C(n+1). They also give an incorrect value for C(8).

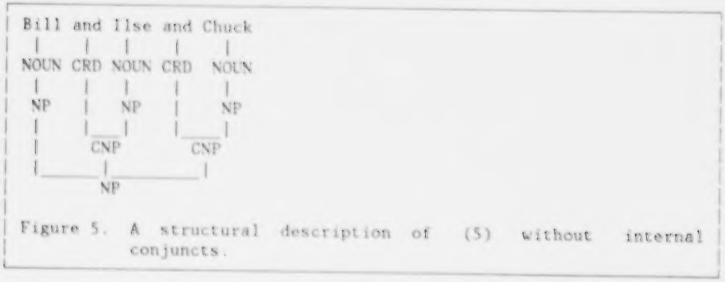

Figure 5. A structural description of (5) without internal conjuncts.

We obtain a grammar that is able to associate flat structures as well as nested ones with coordinate compound constructions by replacing rule (a) in the grammar in Figure 1 with the schema in (a').

(a') NP ---> NP (CNP)* CNP

However, the degree of amphibology predicted by this new grammar is much greater than that predicted by the grammar in Figure 1, as shown in the table in Figure 6 we refer to these numbers as 'generalized Catalan' numbers.[*]

Number of conjoins	Number of structures
2	1
3	3
4	11
5	45
6	197
7	903
8	4279
9	20793
10	103049

Figure 6. Number of structures associated with coordinate compound phrases generated by the grammar in Figure 1, with rule schema (a') replacing rule (a), as a function of the number of conjoins.

The values in Figure 6 may be calculated by the following tedious, but straightforward, method. Let S(n) be the number of structures asso-

[*] I thank Andy Neff for his help .n determining these values.

ciated with a string generated by the grammar in question with n conjoins, and let S(1) = 1. Suppose we know the values of S(n) for all n up to some number k. We determine S(k+1) as follows. First, let m(i), 1 <= i <= k, be the number of daughters of the root node that dominate exactly i conjoins. Then we have the equality in (6), since the number of conjoins of all the daughters of the root node must be exactly k+1.

$$(6)\quad \sum_{i=1}^{k} i*m(i) = k+1$$

To illustrate the general problem of how to calculate S(k+1), consider how we would determine the value of S(4), based on the values of S(1), S(2) and S(3). In Figure 7, are listed all the combinations of values of m(i) that satisfy (6).

Case	m(1)	m(2)	m(3)
1	4	0	0
2	2	1	0
3	1	0	1
4	0	2	0

Figure 7. Combinations of m(i) for k=3 satisfying the equality in (6).

In particular, consider case (3) in Figure 7. How many structural descriptions correspond to that case? The root node has two daughters; one contains one conjoin, the other contains three. These may be arranged in two different ways. The daughter with one conjoin may have S(1)=1 different structural arrangements. The daughter with three conjoins may have S(3)=3 structural arrangements. Therefore, the total number of structural descriptions associated with this case is 2*1*3=6. The numbers of structural descriptions corresponding to the other cases are computed in a similar way.

The general formula for computing S(k+1) is given in (7).[5]

$$(7)\quad S(k+1) = \sum \binom{m(1)+\ldots+m(k)}{m(1)\ \ldots\ m(k)} \prod_{i=1}^{k} S(i)^{m(i)},$$

for all k-tuples <m(1),...,m(k)>, that satisfy (6).

[5] Janda (1975) describes a program for calculating S, but it gives incorrect results for values of k greater than 7.

3. Flat Structure and Mixed Coordinators

In section 2, we illustrated flat coordinate compound structures with examples that all contained exactly the same coordinators, but the grammar that we developed in that section permits phrases with mixed coordinators, such as (1), to have flat structures as well. That is, that grammar assigns three distinct structural descriptions to (1), not two. However, it does not appear that the flat structure of (1) can be directly assigned a meaningful interpretation. Its status is rather like that of unparenthesized arithmetic expressions with nonassociative operators, such as (8), that are permitted by the syntax of programming languages.

(8) 2 + 3 * 6

Such expressions cannot be evaluated as such, since they do not tell us which operation (addition or multiplication) to apply first. Only expressions with operands grouped by parentheses can be interpreted, such as (9) and (10).

(9) (2 + 3) * 6

(10) 2 + (3 * 6)

The fact that (8) has no interpretation as it stands, however, does not mean that it cannot be assigned an interpretation by convention. For example, it may be decided to group the operands in expressions like (8) pairwise from left to right, thus giving (8) the interpretation of (9). Or it may be decided that multiplication should have 'priority' over addition, thus giving (8) the interpretation of (10). Whatever is decided about the interpretation of (8), all three expressions (8)-(10) are syntactically well-formed in the programming languages in which they occur, and none of them is ambiguous.

Returning to natural-language examples like (1), we see that we have no uniform convention for interpreting flat structures with mixed coordinators in natural languages. In the case of (1), we may interpret it either as (2) or (3), or give it no interpretation at all. In other cases, we may be guided by our experience to favor one or another interpretation. For example, when confronted with a restaurant menu that offers us the choices in (11) and (12), we most likely would interpret (11) as (13) and (12) as (14), respectively, on the grounds that soup and crackers are generally served together and that tea or coffee is generally offered as a choice together with dessert.

(11) soup and crackers or juice

(12) dessert and tea or coffee

(13) either soup and crackers or juice

(14) dessert and either tea or coffee

In an interesting set of experiments, Streeter (1978) showed how arithmetic expressions like (8) can be reliably disambiguated in speech by means of durational and intonational cues. English expressions like (1) can be similarly disambiguated. Using a broken vertical bar to indicate a phrasing cue (prolongation of the immediately preceding phrase and/or an intonational break), (1) can be phrased in the three ways indicated in (16)-(18).

(16) Bill and Ilse | or Chuck

(17) Bill | and Ilse or Chuck

(18) Bill | and Ilse | or Chuck

The phrasing in (16) has the interpretation of (2); (17) has the interpretation of (3); and (18) has the interpretation of the flat structure. (Note that (18) has the same interpretation as (1) said without any internal phonological phrasing.) If English intonation could be reliably encoded in writing, then (1) would no longer be an amphibology; each of the spoken versions (16)-(18) would have its own exact written counterpart.

4. On the Distinctions Rendered by English Phrasing

However, English phrasing is not adequate to distinguish among all the possible structures that the phrase-structure schema in section 2 assigns to coordinate compound expressions with four or more conjoins. Consider the following example, with four conjoins.

(19) Bill and Chuck or Ilse or Terry

Example (19) may be said without internal phrasing (in which case, like (1), it is interpreted as having flat structure), or it may be said with any of the internal phrasings in (20)-(26).

(20) Bill | and Chuck or Ilse or Terry

(21) Bill and Chuck | or Ilse or Terry

(22) Bill and Chuck or Ilse | or Terry

(23) Bill | and Chuck | or Ilse or Terry

(24) Bill | and Chuck or Ilse | or Terry

(25) Bill and Chuck | or Ilse | or Terry

(26) Bill | and Chuck | or Ilse | or Terry

These phrasings have interpretations that correspond to the bracketings in (27)-(33).

(27) (Bill) and (Chuck or Ilse or Terry)

(28) (Bill and Chuck) or (Ilse or Terry)

(29) (Bill and Chuck or Ilse) or (Terry)

(30) (Bill) and (Chuck) or (Ilse or Terry)

(31) (Bill) and (Chuck or Ilse) or (Terry)

(32) (Bill and Chuck) or (Ilse) or (Terry)

(33) (Bill) and (Chuck) or (Ilse) or (Terry)

The crucial observation is that intonational cues are not used to indicate more than one level of embedding; their only function is to chunk the total expression into subphrases at the first level of embedding. Accordingly, in a phrase of n conjoins, intonational cues can be used to distinguish at most 2**(n-1)-1 different structures, far fewer than the number of structures that are theoretically possible given the grammar in section 2. To indicate subordination of conjoins, one must resort to paraphrase. For example, the logical structure in (34) may be expressed as in (35).

(34) (Bill and (Chuck or Ilse)) or (Terry)

(35) either Bill and either Chuck or Ilse or Terry

However, while the use of either to mark the beginning of a disjunction with a correlative occurrence of or is unrestricted in English, the corresponding use of both with correlative and is limited to phrases with exactly two conjoins. Hence there is no easy way to produce many of the logical structures predicted by the grammar in section 2 in English. Moreover, phrases with nested occurrences of either...or and both...and quickly become difficult to understand because of center embedding.

5. Serial Coordination

English also has a coordinate compound construction which exhibits flat structure only; it is illustrated in (36).

(36) Bill, Ilse or Chuck

In this construction, which we call serial coordination, the coordinator appears between the last two conjoins only, while (in written English) a comma, or under certain conditions, a semicolon, separates the other conjoins. Ignorning punctuation, we can account for serial coordination by adding to the grammar in section 2 the schema in (a").

(a") NP --> NP (NP)* CNP

The coordinator that appears between the last two conjoins is understood as connecting all of the conjoins in the construction; thus (36) is logically equivalent to (37) (with flat structure).

(37) Bill or Ilse or Chuck

Serial coordinate structures may enter into larger constructions, as in the following examples.

(38) Bill and Ilse, Terry or Chuck

(39) Bill, Terry and Ilse or Chuck

(40) Bill and Ilse; Chuck, Terry or David; and Cathy, Arnold and Mike

(41) Bill and Ilse; Chuck; Terry; or David and Cathy, Arnold and Mike

Example (38) may be read in two different ways, depending on whether Bill and Ilse occurs as a phrase in it (this would be indicated in speech by the absence of an intonational boundary between Bill and and). If it does, then the example as a whole is understood as a disjunction of three things: Bill and Ilse, Terry, and Chuck. If it doesn't, then the phrase is understood as the conjunction of two things: Bill and Ilse, Terry or Chuck. Similarly, example (39) may also be read in two different ways, this time depending on whether Terry and Ilse appears as a phrase in it. Next, example (40), as punctuated, is unambiguously interpreted as a conjunction of the three phrases separated by semicolons. If the first semicolon were replaced by a comma, then the phrase Bill and Ilse would be construed as the first of the disjuncts ending with David Finally, example (41), as punctuated, is unambiguously interpreted as a disjunction made up of the four parts Bill and Ilse, Chuck, Terry, and David and Cathy, Arnold and Mike.

The distinctive use of the punctuation marks in serial coordination in written English to some extent parallels the use of intonational cues to distinguish among various interpretations of ordinary coordination in spoken English. Moreover, the judicious combination of commas and semicolons in serial coordination is able, under certain circumstances, as in (41), to indicate up to two degrees of embedding, but no more. If the comma and the semicolon are used together, then the semicolon may be used to indicate the first level of embedding, and the comma to indicate the second level. I do not believe, however, that examples of serial coordination, like (41), can also be spoken so as to indicate the double embedding of coordinate structures.

6. Conclusions

The treatment of coordinate compounding by means of simple phrase-structure rules predicts much more amphibology than is in fact found in natural-language coordinate structures. Coordinate compounding in English without the use of correlative markers such as either and both is limited to one degree of embedding, except under special circumstances

involving serial coordination, in which it is limited to two degrees of embedding.[6] Thus the degree of amphibology in coordinate compound structures is expressed by neither the Catalan numbers discussed in Section 1, nor the generalized Catalan numbers discussed in Section 2, but (ignoring the possibility of double embedding in serial coordination) by one less than 2 raised to the power of one less than the number of conjoins. In careful spoken English, moreover, no coordinate compound expression of the type under discussion here is structurally ambiguous, since the structure can be uniquely indicated by the intonational phrasing.

The restriction against multiple embedding of coordinate compound structures can be expressed directly by means of a finite-state grammar, or by means of an augmented phrase-structure grammar that keeps track of the degree of embedding of coordinate compound structures. If the grammar is also permitted to perform the structure building characteristic of the algorithm that associates tree diagrams with derivations, then an elegant statement of the rules of grammar needed to characterize the structures of coordinate compounds can be achieved, without the need for rule schemata (cf. Jensen in press). Thus, the time-honored Chomskyan strictures against the tracking of derivations and against structure building (cf. Chomsky 1965) by phrase-structure rules have prevented linguists until now from achieving adequate characterizations of a wide range of linguistic phenomena.

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[6] The limitation on embedding of coordinate compounds is inconsistent with the principle of coordinate compounding discussed in Langendoen and Postal (1984), which is necessary to our demonstration that the collections of expressions of a natural language is a proper class. However, the limitation does not affect our demonstration that the number of expressions of English is of the order of the continuum, and hence nondenumerable.

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Closure Duration in the Classification of Stops: A statistical analysis

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[Some thirty years ago, Ilse Lehiste and I were fellow students in Ann Arbor, working in the laboratory of the late Gordon E. Peterson, our teacher. After that, for several years we were colleagues in Columbus, laying the foundation for an embryonic Department of Linguistics. Through these decades, her friendship has always been a source of comfort for me, and her scholarship a standard for emulation. My co-authors and I are pleased to have this opportunity to honor her. This little essay is on phonetics, a field to which she has contributed so much. - WSYW]

Stop consonants have been traditionally described by the terms voiced-voiceless, aspirated-unaspirated and tense-lax (fortis/lenis). It has been suggested (Lisker & Abramson 1964) that all the acoustic properties of stops are consequences of a change in the timing relationship between the release of a closure and the onset of vocal fold vibration.

The technical term VOT (voice onset time) has been applied to this timing relationship. However, the VOT approach has difficulties in a number of languages, e.g., Korean (Kim 1965). It is obvious that if the contrast of stops does not exist along voice-voiceless and aspirated-unaspirated dimensions, the VOT method will be unable to capture the difference.

Another approach for classifying stops was proposed by Halle and Stevens in their 1971 article that fundamental frequency varies as a function of vocal fold tension. It has been shown (Lehiste & Peterson 1961) that a voicing distinction of stop sound in prevocalic position can affect the F0 of the following vowel. Further, although VOT was very similar for the initial voiced stops and the voiceless stops after an s, the F0 were very dissimilar (Reeds & Wang, 1951 and Chae, 1984). It seems that this might provide us with an alternate method for classifying stop sounds which do not have the contrast in voicing and aspiration.

Are these two approches sufficient to classify stops in any language? Toward answering this question a dialect of Chinese is used. The Wu dialect

is one of seven major dialects of Chinese. It has three types of stop sounds (traditionally they are described as voiceless unaspirated, voiceless aspirated and voiced). Y. R. Chao called this "tripartite division of initial stops" (1928). As early as in the 1920's F. Liu found that the voiced stops in Wu are actually not voiced. It was also noticed by Chao that when this sound occurs in an intervocalic position it is fully voiced.

In recent years there have been two acoustic studies of Wu stops by Cao (1982) and Shi (1983). Both of them show that the VOT does not differ significantly between the two unaspirated stops. Here are the VOT values reported by Shi. (Cao did not supply any quantitative data)

b	p	d	t	g	k
7.5	6	7.5	7	20	16

Shi also measured the fundamental frequency of the following vowels to examine the effect the stops have on their following vowels. But this approach was not successful for two reasons: 1) Since Wu is a tone language the change in fundamental frequency is mainly a tonal phenomenon, and 2) The stops are in complementary distribution. As a result of historical change, the so-called voiced stops never occur in the same tone with voiceless stops. Thus Shi was unable to determine if the F0 differences were due to the tone or due to the consonants.

Because of the problems discussed above, we need to find some other acoustic cues besides VOT and F0 to study the stop sounds in Wu. We know that stop sounds are produced with the vocal tract entirely occluded. When the vocal tract is occluded at some point within the oral cavity the air escaping from the lungs is trapped behind the constriction, thereby causing a build-up of oropharyngeal air pressure.

The following acoustical parameters appear to be important cues to identifying stop consonants:

1) Duration of the closure
2) Presence of the voicing during the closure phase
3) Voice onset time
4) Duration of the noise burst
5) The perturbation on the following vowels

The closure durations of voiceless stop sounds are greater than for their voiced counterparts. This fact was observed and studied by Lisker (1957), in a study to distinguish stop consonants in word-internal position.

But in initial position, the difficulty to get the duration of the closure is that if the stop sound is voiceless during the closure and at an initial position, it will result just as silence in the waveform. Although physically there is a starting point of the closure, we can not determine it from the waveform.

In order to get the starting point of the closure, we put the syllables with the stops we are interested in in a carrier phrase. Thus, we can use the end of the preceding syllable as a reference point to get the closure duration of the stop.

The data we use is taken from Shanghai speech, which belongs to the Wu dialect group. It is not the same variety of Wu studied by Shi and Cao, but the stop types are the same (Chao 1928). Some relevant phonological features of Shanghainese are introduced below.

Some phonological features of Shanghainese:

There are three different types of stops: one is voiceless and aspirated, and two are phonetically unvoiced and unaspirated. But traditionally one is called voiceless and the other one is called voiced. All three types of stops appear at three articulatory places: bilabial, alveolar and velar. Here we will not consider the aspirated ones, since they do not pose a problem for traditional methods of classification. For convenience, we use the symbols p, t and k for the voiceless stops and b, d and g for the so-called voiced stops.

There are five tones, three of them are long and two of them are short. Their phonetic values in a five height system (5 is the highest and 1 is the lowest) are: 1. 51 2. 334 3.113 4. 5(short) 5. 23(short). The two series of unaspirated stops are distributed in different tones. P, t and k's only occur with tone 1,2, and 4 (high register), and b, d and g's only occur with tone 3 and 5 (low register).

Experiment 1:

The two types of stops at three articulatory positions with a vowel, [e] for long tones and [ə] for short tones, are selected for each of the five tones. All the syllables are real words in Shanghainese.

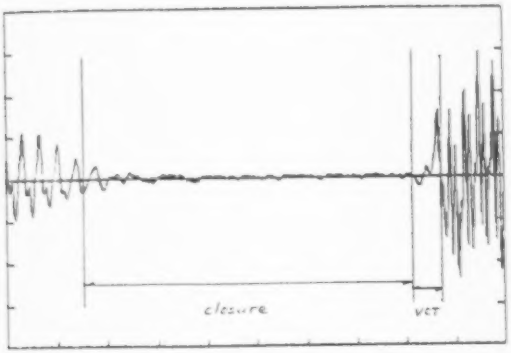
Tone	1	2	3	4	5
bilabial	pe 批	pe 拔	be 培	pə 搬	bə 拔
alveolar	te 低	te 特	de 狄	tə 答	də 达
velar	ke 阁	ke 揭	ge [illegible]	kə 葛	gə 轧

Each word is pronounced ten times. The total number of tokens is 3x5x10=150. The recordings were made in a sound booth. The experimental utterances were displayed on a IBM PC-AT screen by using the ILS (Interactive Laboratory Systems) software made by Signal Technology Inc.

In order to get the closure time of the articulator, the tokens are placed in carrier phrases /əʔ __ 1? pi/ (which means "Say __ once." in English). Three measurements are made for each stop, as shown in Figure 1 below. Each division on the x-axis marks off 20 msc.

The starting point of closure in the waveform was determined on the basis of both a sudden decrease in amplitude, and a loss of high frequency components. As we can see from Figure 1, the voicing containing energy at the fundamental frequency and F1 continues past the start point of closure for several periods. The end point of the closure was determined on the appearance of the noise type waveform. And the voice onset point was determined on the appearance of periodic waveform, from the starting point to the end point of the closure is taken to be the closure of articulator. The duration from the end point of the closure to the voice onset point is the

traditional VOT. After the voice onset point the durations of five successive periods were also measured.



All the calculations are done by using the Statistical Analysis System (SAS). The results for monosyllabic word in a carrier phrase, listed by tones, are shown below:

TONE 1

variable	n	mean	S.D.	min-value	max-value
clo	30	157.80	15.25	128.00	191.50
VOT	30	16.27	11.94	5.90	48.00
F01	30	5.85	0.70	4.50	7.10
F02	30	6.42	0.38	5.70	7.10
F03	30	6.31	0.41	5.60	6.90
F04	30	6.35	0.36	5.70	7.00
F05	30	6.38	0.35	5.80	7.10

TONE 2

variable	n	mean	S.D.	min-value	max-value
clo	30	150.60	15.39	116.60	178.50
VOT	30	19.45	9.25	7.20	42.30
F01	30	7.35	0.60	6.00	8.60
F02	30	8.13	0.63	7.00	9.20
F03	30	8.23	0.54	6.60	9.30
F04	30	8.33	0.63	7.10	9.40
F05	30	8.45	0.58	7.20	9.60

TONE 3

variable	n	mean	S.D.	min-value	max-value
clo	30	126.01	19.92	82.60	156.10
VOT	30	24.33	10.70	11.50	45.70
F01	30	8.58	0.92	7.20	11.40

F02	30	9.84	0.55	8.90	11.20
F03	30	10.18	0.47	9.50	11.40
F04	30	10.29	0.37	9.60	11.30
F05	30	10.31	0.36	9.60	11.20

TONE 4

variable	n	mean	S.D.	min-value	max-value
clo	30	156.26	14.15	132.40	179.70
VOT	30	15.52	9.02	7.90	34.00
F01	30	6.03	0.36	5.30	6.60
F02	30	6.78	0.34	6.10	7.40
F03	30	6.73	0.22	6.30	7.20
F04	30	6.76	0.23	6.20	7.20
F05	30	6.81	0.23	6.20	7.20

TONE 5

variable	n	mean	S.D.	min-value	max-value
clo	30	125.98	13.65	99.60	149.80
VOT	30	21.43	12.24	6.90	59.40
F01	30	7.89	0.66	6.00	9.50
F02	30	9.05	0.43	8.30	10.10
F03	30	9.23	0.38	8.60	9.80
F04	30	9.19	0.38	8.30	10.20
F05	30	9.17	0.33	8.50	9.80

From these results above we can see that:

1) The average value of closure durations of p, t, and k in tone 1,2 and 4 are longer than those of b, d and g in tone 3 and 5. In descending values, these are: 157.8(1), 156.3(4), 150.6(2), 126.0(3) and 126.0(5). The difference between these two types of stops is approximately 30 milliseconds.

2) All the VOT values are positive. The average value of VOT values in five tones from shortest to longest are 15.52 (4), 16.27(1), 19.45(2), 21.43(5) and 24.33.(3) separately. It means that these two types stops are both voiceless stops. On the average, the b, d and g have a longer VOT value than the p,t and k, but the difference is relatively small. The difference is about 6 milliseconds ((21.43+24.33) / 2-(15.52+16.27+19.45)/3=5.8).

3) The duration of the five periods are longer for b,d and g.

4) Although the averages are different, from the ranges of each variable we can see that they overlap with each other.

The maximum value of a variable with a smaller average is always above the minimum value of a corresponding variable with a larger average. For example: The closure duration in tone 2 is 150.6, it is longer than the average closure duration in tone 3, which 126.0. But the minimum value in tone 2 (116.6) is smaller than the maximum value in tone 3 (166.1). There is no clear boundary between these variables. By looking at average and range, we would not be able to determine how many types of stop sounds there are.

In order to tell the statistical significance of these numbers, a multivariate statistical method Hotelling's T-square is applied to test the

differences between all 10 possible pairs (nx(n-1)/2, n=5). The formula we used is:

$$T^2 = [N_1 N_2 / (N_1 + N_2)] (\bar{X}_1 - \bar{X}_2)' S_c^{-1} (\bar{X}_1 - \bar{X}_2)$$

The corresponding F value is calculated in the following way:

$$F = (N_1 + N_2 - p - 1)\, T^2 / [p(N_1 + N_2 - 2)]$$

with p and $N_1 + N_2 - p - 1$ df $\quad p = 7,\ N_1 = N_2 = 30$

The result is shown below:

	1	2	3	4
2	40.82			
3	277.79	36.38		
4	6.72	30.06	332.02	
5	144.64	16.35	29.26	159.96

$F(7,50) = 3.02$ ($\alpha = 0.01$)

All the F values indicate that none of them is the same, since all the F values are larger than 3.02. It cannot be the right result since there are only two types of stops. We are using three different measurements here, the closure duration, VOT and F0. In order to determine which parameter contributes to the difference shown by the F values, a post-hoc T-square test is applied.

$$F_{sub} = n \frac{(\bar{X}_n)^2}{S_{nn}}$$

Here are the results:

Tones	12	13	14	15	23
clo	0.42	6.16	0.02	9.28	3.67
VOT	0.17	0.97	0.01	0.35	0.46
F01	7.68	21.40	0.18	17.18	3.19
F02	20.79	102.07	1.94	81.23	16.09
F03	24.38	148.09	3.12	105.41	22.95
F04	28.77	224.40	3.95	114.11	27.35
F05	34.98	228.01	3.97	125.72	28.49

Tones	24	25	34	35	45
clo	0.28	5.50	5.88	0.00	9.12
VOT	0.35	0.06	1.52	0.12	0.58
F01	6.89	1.02	25.79	1.44	23.69
F02	13.51	5.59	86.37	4.91	55.27
F03	18.74	6.89	168.95	9.39	124.46
F04	20.47	5.24	244.71	15.10	111.96
F05	20.15	4.54	257.95	20.97	133.37

The first row are the F values of the closure durations. It tells us that there is no significant difference between the pairs 12, 14, 24 and 35 (<3.02). But the differences are significant for the 13, 15, 23, 25, 34 and 45 pairs. We have mentioned that traditionally the stops in tone 1, 2 and 4 are called voiceless and the stops in tone 3 and 5 are called voiced (although they are voiceless also). Here, the F values for the closure duration clearly indicate the same classification that the stops in tone 1, 2 and 4 are one type which significantly differ from the other type of stops that occur with tone 3 and 5. The question whether the two types of unaspirated stops are the same is answered.

The second row is the F values of VOT. None of these values reached a significant level. From the average data, we know that the p, t and k's have a shorter VOT than their counterparts. It indicates that VOT is not a significant parameter for distinguishing these two types of stops. It proves that the difference of these two types of stops in Wu is neither a voiced-voiceless nor a aspirated-unaspirated contrast.

From row 3 to row 7, are five periods after the release of the closure. Except four of them, first and second periods in pair 17 and the first period in pairs 58 and 68, all the other forty-six (92%) F values show that they are significantly different. These F values show that fundamental frequencies are not good variable which can be used to distinguish the stop types in tone languages like Wu. The F0 basically is a tonal phenomenon. Here we cannot be sure of how much the F0 difference is due to the tonal difference and how much due to the difference of stop types. We say this difference basically is a tonal difference simply because that they fit the traditional description of the tonal values well. Let us look at the table below:

tone	value	average duration of five periods
1	51	6.26 (160 Hz)
2	334	8.10 (123 Hz)
3	113	9.84 (102 Hz)
4	4(short)	6.63 (151 Hz)
5	23(short)	8.91 (112 Hz)

It is obvious that when the duration of the period is shorter, the rate of vibration is higher. Although there may exist some perturbations on the vowels caused by the stop sounds, we will not be able to distinguish them because they are dominated by much stronger tonal affects. And this also explains why the F values are all different in the tests with all 7 variables, because the results are strongly influenced by five F0 variables which basically are the tonal differences.

Experiment 2:

The two types of unaspirated stops also can occur in word-internal position in a disyllabic words. Because of the rules of tone sandhi, two unaspirated stops can and only appear in the same disyllabic tones. All the tones lose their contrast in the second syllabic position. Thus, five monosyllabic tones yield five corresponding disyllabic tones. The tonal values in five level system are:

tone	value
1	55 + 31
2	33 + 44
3	11 + 44
4	4(short) + 55
5	2(short) + 23

In these disyllabic words, the two types of unaspirated stops clearly contrast in voicing. The comparisons we make here apply to stops within the same tone only. The first syllables of these disyllabic words are /se/, /tciu/, /liang/, /tchi/ and /lo/. All of them are number words, they are 'three', 'nine', 'two', 'seven' and 'six' separately. Each of them is used to represent a different tone. Three pairs of words with the stops in question at three articulatory places are chosen for the second syllable. They are: /pe/vs./be/, /te/vs./de/ and /kwe/vs/gwe/. The total number of tokens for this experiment is 5x2x3x10=300.

From the waveforms, it is clear that most of the word-internal b, d and g's have voicing during the entire closure, but not the p, t and k's. Thus, in this word-internal position the difference of two stop types also appears as a voice-voiceless contrast. This is the reason for many previous studies to call them "real voiced stops"(Chao, Cao, Shi). But the word-internal voiced-voiceless contrast is not consistent in all tonal environments. The interesting thing is that in disyllabic words with tone 5 (11 + 23), neither stop shows any voicing.

This is not difficult to understand. Although stress contrasts have not been described in Wu dialects, in fact they exist and correlate well with tones within words. The contour of intensity has a similar change as the pitch contour. So the independence of the stress usually will be ignored. But here the relationship between the pattern of stress and voicing causes our attention. In all five disyllabic tones only tone 5 has the weak+strong pattern, while others have the strong+weak pattern instead. The lack of voicing in tone 5 indicates that if the second syllable is stressed the contrast between these two stops is still pretty much like that found at initial position. Thus it is necessary to measure the closure duration and F0 in order to classify these stops.

In the word-internal position, if there is voicing for b, d and g's, the duration of voicing equals the duration of closure duration and equals the VOT duration. So here we only measure the closure duration for both p, t and k's and b, d and g's. The durations of five periods after the burst are also measured like what we did in monosyllabic words. The results are shown below:

	p,t,k			b,d,g		
Tone 1	mean	s.d.	range	mean	s.d.	range
clo	123.18	12.68	97.5-146.9	70.61	11.01	53.4- 92.5
F01	12.54	1.22	9.6- 14.5	12.75	1.22	10.6- 15.4
F02	13.76	0.82	12.2- 15.0	13.74	1.09	11.8- 16.4
F03	14.40	0.58	13.0- 17.0	14.16	0.96	12.2- 16.4
F04	14.90	1.22	13.0- 18.0	14.58	1.06	12.8- 16.8
F05	15.60	1.28	14.0- 18.8	15.16	1.22	13.2- 17.4

Tone 2	mean	s.d.	range	mean	s.d.	range
clo	129.33	15.92	110.7-154.6	82.39	11.15	59.7-106.9
F01	4.86	0.54	3.8- 6.0	5.48	0.76	4.0- 6.8
F02	5.32	0.52	4.2- 6.0	5.98	0.54	4.6- 6.8
F03	5.40	0.52	4.4- 6.4	5.98	0.52	4.6- 6.6
F04	5.40	0.50	4.4- 6.0	5.84	0.56	4.6- 6.6
F05	5.42	0.52	4.6- 6.2	5.80	0.48	4.6- 6.6

Tone 3	mean	s.d.	range	mean	s.d.	range
clo	133.49	17.38	90.9-163.2	105.86	22.85	72.1-130.2
F01	5.08	0.74	3.4- 6.2	5.46	1.38	5.0- 7.6
F02	5.80	0.66	4.4- 7.0	6.76	1.42	5.4- 7.8
F03	5.92	0.68	4.6- 6.8	6.78	1.46	5.4- 7.8
F04	5.82	0.64	4.6- 7.0	6.82	1.46	5.6- 7.8
F05	5.82	0.68	4.6- 7.0	6.78	1.44	5.6- 7.8

Tone 4	mean	s.d.	range	mean	s.d.	range
clo	186.60	26.39	143.0-240.2	143.04	26.45	90.7-169.8
F01	4.93	0.60	3.6- 5.8	5.67	0.71	4.2- 6.8
F02	5.56	0.57	4.2- 6.4	6.2	0.65	4.8- 6.8
F03	5.71	0.58	4.2- 6.4	6.15	0.59	4.8- 6.8
F04	5.68	0.56	4.6- 6.4	6.11	0.55	4.8- 6.6
F05	5.69	0.55	4.4- 6.4	6.08	0.58	4.8- 6.8

Tone 5	mean	s.d.	range	mean	s.d.	range
clo	217.81	18.80	189.6-268.8	136.64	18.22	110.0-161.0
F01	6.83	0.73	5.4- 8.0	7.60	0.94	5.8- 9.4
F02	7.13	0.64	5.8- 3.9	8.02	0.89	6.4- 10.0
F03	7.18	0.69	5.8- 8.2	8.11	0.84	6.2- 9.6
F04	7.25	0.70	5.8- 8.0	8.12	0.87	6.4- 9.2
F05	7.31	0.72	5.6- 8.4	8.04	0.78	6.4- 9.2

N = 30

The results can be summarized as follows: 1) The average value of closure duration for p, t and k's are longer than [illegible] for b, d and g's in the same tones. 2) The difference is relative not absolute. It means that the p, t and k's has a longer duration than b, d and gs only in the same tonal environment, not all the closure durations for p, t and k's are longer than b, d and g's. For example, the closure duration for p, t and k's in tone 3 is 109.5 msec. which is shorter than the closure duration for b, d and g's in tone 4 which is 143.0 msec.. Here we do not wish to explain these compensatory lengthening of the closure duration caused by the duration of preceding syllables, because the comparisons we interested in are only within each tonal category.

3) The durations of five periods on the average are shorter for the p, t and k's in tone 2, 3, 4 and 5, but it is in the opposite way in tone 1. 4) like what we see in monosyllabic tones, the phenomenon of overlapping is also in the data of disyllabic words.

In order to explain these numbers, the same Hotelling's T-square test is applied to the differences of two types of stops within the same tone. The

results (F values) are listed below:

Tones	1	2	3	4	5
F-value	44.78	28.77	24.40	11.52	54.50

$F(6,50) = 3.19$ ($\alpha = 0.01$)

These F values tell us that in all the disyllabic tones the p, t and k's are significantly different (> 3.19) from the b, d and g's in the same tone. Here we like to know the contribution of closure duration and F0 to the total differences. The post-hoc test shows the F values below:

	1	2	3	4	5
clo.	43.55	26.65	4.23	6.21	43.93
F01	0.02	1.89	3.51	2.88	1.90
F02	0.11	2.58	1.72	2.49	3.01
F03	0.25	1.96	1.64	1.24	3.32
F04	0.28	1.52	1.82	1.32	2.79
F05	0.34	1.33	1.63	1.11	2.14

The first row is the F value of the closure durations which shows us that the closure duration of two different stops are significantly different(> 3.19) in all five disyllabic tones. From second row to sixth row are the F values for five periods after the release of the closure. Except the F01 in tone 3, all the F values shows that the difference caused by the different stops are not significantly different(< 3.19), although the average value suggests that F0 after p, t and k's are higher than b, d and g's. Again, from another angle it shows that, as we had found in monosyllabic words, the tonal influence is dominant.

Discussion:

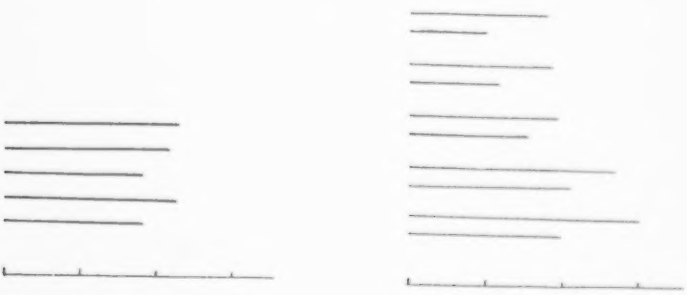
From the data and statistical results, we see that the closure duration efficiently classifies between p, t and k's and b, d and g's. On the other hand, the traditionally used acoustic cues VOT and F0 are not satisfactory here. From an articulatory point of view, both methods paid attention only to the glottal gesture, VOT refers to the relationship between the supraglottal closure release (the end point of closure duration) and the onset time of vocal folds vibration. And the F0 is also only concerned with the rate of vibration of the vocal folds after the closure release.

A complete account of stop sounds the closure period must also be taken into account. But in many acoustic studies this main part of stop sound production has been ignored.

Speech sounds differ from each other along several dimensions. This means that in some languages the stop sounds can be classified in more than one way. In English, for instance, the stops differ not only along the voiced-voiceless dimension, but along the tense-lax dimension as well. We have no reason to say that voice-voiceless or tense-lax is the only or the best way to do the classification in all the languages. But, as shown in Figure 2 below, whether the stops have voice-voiceless distinction or not and no matter what

phonological positions they occur in, closure duration is always a reliable indicator.

Figure 2 has two displays. The length of the lines in Figure 2 shows the closure durations of the stops, each line showing the average over 30 samples. The divisions on the base line marks off increments of 50 msc. In the left display of monosyllabic samples, the top line shows that the average closure duration for p, t, and k with tone 1 is 157.8 msc, as reported in Experiment 1 earlier. Since both types of stops occur in the second syllable, the right display shows the line in pairs, as reported in Experiment 2 earlier. The upper line in the top pair shows that the average closure duration for p, t, and k with tone 1 is 123.2 msc.; the lower line in the top pair shows that the average duration for b, d, and g with tone 1 is 70.6 msc.



Here we used closure duration to study the stop sound in a tone language and got a clear classification between the two types of unaspirated stop sounds. Since the stops at initial positions are all voiceless, the name "voiced", strictly speaking, is a misnomer. The contradiction between the conventional name of a sound and phonetic reality of a sound is clear in this case. The right result from the measurements of closure duration does not necessarily indicate that the contrast between these stops only exists in one variable. Rather, it is a multidimensional phenomenon. But the importance of each variable is different. This was evalueated by the means of the statistical tests discussed above.

Conclusion:

It turns out that the term "stop" was aptly chosen, since the most effective classifier of stop sounds is the duration for which the sound is stopped. A recent study on the aerodynamics of Korean stops (Dart, 1987) reports that the fortis stops show a higher intraoral pressure. But the fortis stops are produced with longer closure durations as well. If we assume that the amount of air flow from the lungs to the speech tract remains approximately constant, then the higher pressure is simply a consequence of the longer closure duration.

Although the b, d, g discussed here are sometimes voiced, in word-internal positions, they are not voiced in general. Even though both types of stops are unvoiced and unaspirated, the b, d, and g have a significantly shorter closure duration.

Another central aspect of this study is that we tried to take fully into account that fact that a contrast is typically implemented multi-dimensionally, that is, simultaneously along several phonetic parameters. The situation is more complex in tone languages, such as the Shanghainese reported here, where one parameter, in this case F0, has many uses (Wang, 1972). In such situations, statistical procedures are especially necessary for evaluating the relative importance of the various parameters for different contrasts.

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Silent Features and Syntactic Analysis

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1. What are silent features?

The use of features to distinguish within a given class of expressions various subclasses is by now an accepted practice among syntacticians, as it has been for some time in phonology. For instance, if one wants to distinguish mass nouns from count nouns, it is convenient to add a feature [COUNT] to the grammar and to mark some of the nouns as [+COUNT] and the rest as [-COUNT]. Usually, features do not create a strict partitioning of the set of expressions to which they apply. For example, some nouns are both [+ COUNT] and [- COUNT]. Some overlap of the extensions of the feature values, so to speak, must be allowed. Note that this is in general not true in phonology. A phoneme is marked either positively or negatively for a given feature, but not both. However, a tacit assumption among syntacticians seems to be that the values for a syntactic feature may not pick out sets which completely overlap with one another, i.e. identical sets. One simply does not find syntactic descriptions which employ a binary feature [@F] such that every expression which is [+F] is also [-F]. In this paper, features such as [@F] will be referred to as "silent features".

Silent features should not be confused with the "covert categories" of Zwicky (1986). The latter are categories which are not expressed by the rules of inflectional morphology. For instance, the difference between past and present tense is morphologically marked in English, and hence we can say that [TENSE] is an overt category of English. On the other hand, the difference between transitive and intransitive verbs is not expressed by the rules of English inflectional morphology, and so transitivity is a covert category in that sense. However, it is by no means a silent category, of course, since the set of transitive verbs in English is not coextensive with the set of intransitive verbs. It is not hard to see that every silent feature is going to be a covert feature, but not vice versa. In a sense, silent features are maximally covert in the sense that they cannot be distinguished by their "extensions": the set of expressions that bears the + value is the same as the set of expressions that bears the - value. Borrowing a term from formal semantics, we might call such oppositions intensional.

Actually, my concern is mainly with features which are silent in a somewhat weaker sense: namely, features which DO pick out different sets of expressions, but not within certain domains. In other words, my concern is with features which are sometimes silent. Having made the observation that linguists typically do not invoke silent features, one might be tempted to reformulate it as a constraint on syntactic theory (Cooper 1986, cited in Zwicky 1985, 1986 has made just such a proposal):

(1) *The Silent Feature Constraint*

No grammar may employ silent features.

One might argue at this point that this constraint need not be stipulated as a special metacondition on syntactic theory, but could be made to follow from other more general methodological principles, such as Occam's Razor. In the case of features which never do any work, this seems to be the right way to go. However, the more interesting formulation of the constraint is the one which rules out any feature which is silent with respect to some category C (such that C[+F] does not pick out a different set of expressions than C[-F]). I am going to attack the strong interpretation of (1) from another direction. Rather than saying that it is superfluous, I will argue that it is wrong, given some relatively uncontroversial assumptions about syntactic metatheory.

2. German Adjective Agreement in GPSG.

The only current syntactic theory which has something interesting to say about agreement phenomena is Generalized Phrase Structure Grammar (or GPSG). Two basic principles govern the distribution of agreement features in syntactic trees, namely the Control Agreement Principle (CAP), and the Head Feature Convention (HFC). For an extensive discussion of these principles and their formalization, see Gazdar, Klein, Pullum and Sag (1985). For our purposes it is sufficient to note the following. Features come in several flavours: we have so-called head features, foot features and agreement features and general principles as well as language-specific stipulations (which may override the general principles) determine where these features may occur.

The HFC states that the head of a constituent has the same specifications for the head features as the mother node. If number is a head feature, this would explain why a plural noun phrase may not have a singular noun as its head. The CAP states that a functional expression has the same specifications for the agreement features as its argument. So if we view determiners as functional expressions taking nominals as their arguments, and number is also an agreement feature, then it is explained why a plural nominal may not be combined with a singular determiner.

With this in mind, let us now take a quick look at the facts of German adjective agreement, which have been discussed recently by Zwicky in a GPSG setting (see Zwicky 1985). In German, the morphological shape of the adjective is determined not just by the noun (as in most non-Germanic languages with adjective agreement, such as the Romance languages), but also by the type of the determiner. The agreement parameters are number, gender, case and determiner class, as the following examples show:

(2) Number agreement

alter Mann "old man"
alte Maenner "old men"

(3) Gender agreement

alter Mann "old man" (Masc)
alte Frau "old woman" (Fem)
altes Haus "old house" (Neut)

(4) Case agreement

alter Mann NOM
alten Mannes GEN
altem Manne DAT
alten Mann ACC

(5) Det-class agreement

alter Mann (zero determiner: class I)
der alte Mann "the old man" (der: class II)
kein alter Mann "no old man" (kein: class III)

alte Maenner "old men" (class I)
die alten Maenner "the old men" (class II)
keine alten Maenner "no old men" (class III)

(In (5), the first set of examples establishes the difference between class I and III on the one hand and class II on the other hand; the second set shows the difference between class I and class III.) I assume here, with Zwicky, that German noun phrases such as "der alte Mann" have the following structure:

(6)

The agreement facts can be described very elegantly if we assume that NPs are marked for the class-feature of their determiners and that this feature is a head and agreement feature. By the HFC, the N'' and N nodes will have the same specification for this feature as the top node and by the CAP, the determiner and the adjective will receive this specification as well. Indirectly it is brought about that the adjective and the determiner agree. No special stipulations are needed, since all the work is done by the HFC and the CAP. Zwicky, however, does not choose this option, because the determiner-class feature would be silent for the nouns: in German, nouns are not distinguished with respect to the class of the determiner they take. (In addition, Zwicky argues that agreement features must be taken from a universal list of features with semantic content. The determiner-class feature has no clearcut semantic import. However, this requirement seems to be much too strong: it would rule out an account of gender and case agreement in terms of the CAP as well.) Instead of employing the CAP and HFC, Zwicky makes use of a parochial rule of Declension Government and another rule of Declension Inheritance. The first rule gives the N'' sister of the determiner the same specification for the determiner-class feature as the determiner itself and the second one transmits this specification to the AP dominated by the N''.

In this way, there is no need to mark the noun for the feature as well. Note, however, that this analysis not only postulates rules which are not needed under the earlier account but also weakens the interest of the overall theory. The more we let parochial rules do the actual work, the less the explanatory load of the universal principles will be.

At this point, one might remark that my account predicts the possibility of there being a language in which the noun agrees with the determiner. This language would be just like German, except that the agreement feature is not silent. As a matter of fact, such languages exist: Norwegian (Lapointe 1984) and Arabic (Erwin 1963) have definiteness agreement markers on nouns.

To sum up: though the determiner-class feature is not overtly marked in German nouns, there is no reason to assume any incompatibility of this feature with those that characterize nouns. By allowing it as a silent feature, a simple and very general account of the German adjective agreement facts can be given. It seems fair to say that the prohibition of silent features prevents one here to achieve a goal which is considered highly desirable in linguistics, namely to derive the facts of a particular language from universal principles and minimal assumptions about the language in question.

3. Silent features elsewhere.

The determiner-class feature discussed in the preceding section is silent within the set of nouns, but not within the sets of adjectives and determiners. Features which are silent within some sets, but articulated within others are not as uncommon a sight as one might think. Consider for instance English subject-predicate agreement. It is usually said that the finite verb phrase in English agrees in number and person with the subject. However, the distinction between first and second person is overt only when the verb is 'be'. Suppose that we set up the person features in terms of two binary distinctions, one distinguishing the third person from the other two (as seems reasonable on semantic and syntactic grounds) and one which distinguishes the first from the second person. The latter feature is a silent feature for all verbs but 'be'. This verb has a number of properties which set it apart from most other verbs, for instance the fact that it is an auxiliary verb. For all verbs with the feature [-AUX], then, the opposition between first and second person is entirely covert. Within the auxiliary verbs, the opposition is covert for all verbs which do not have the feature [+COPULA].

A very similar story could be told about the English Case features, which must be assumed for noun phrases in order to describe the distribution of pronominals. However, for all non-pronominal NPs, the NOM/ACC distinction is covert and silent. Again, the simplest syntactic description sets up a feature which does not do any work in the majority of cases. One could try to get around this case by stipulating that ACC is the default case (following Gazdar, Klein, Pullum and Sag 1985), and that NOM is assigned only to subject pronouns (NPs with the feature [+PRO] agreeing with a finite verb phrase). However, that would entail a case marking system quite unlike that of other Indoeuropean languages. Furthermore, it still would not mean that there is no NOM/ACC

distinction in the nonpronominal noun phrases. Rather, nominative nonpronominal noun phrases would still be grammatical, but never used in the construction of grammatical sentences. This commits one to a rather baroque ontology, it appears.

My final example comes from English relative clauses. The relative pronoun *who* is usually considered to be a third-person pronoun, but it behaves like a first or second person NP whenever its antecedent is 'I' or 'you' respectively, as the agreement in the following examples shows:

(7) a. I who am your friend
b. You who are so bright

We must assume that the relative pronoun *who* agrees in person (and number) with its antecedent. However, the person and number distinctions are always silent within the class of relative pronouns.

4. Concluding remarks.

The situation sketched in section 2, where a maximally general description of German agreement facts led to a feature distinction which is covert in the class of nouns but overt in the classes of determiners and adjectives has been argued to be not uncommon. Feature systems typically have some redundancy, in the sense that not every combination of feature specifications picks out a different set of expressions.

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